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LECTURES

ON

DISEASES OF THE BRAIN AND NERVOUS SYSTEM,

NOW IN THE COURSE OF DELIVERY IN THE UNIVERSITY OF PARIS.

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LECTURE XII.

ATROPHY OF THE BRAIN AND SPINAL MARROW.

GENTLEMEN,—Having finished in our last lecture the study of hypertrophy of the nervous centres, we now proceed to examine the opposite state, or lesion, which consists in a diminution of nutrition,—in an atrophy of the same part. This atrophy of the nervous centres may result either from a primitive arrest or retardation of development, or from an actual diminution in the bulk and volume of some portion of the cerebro-spinal axis, after it has been fully and perfectly formed. Hence flows naturally a distinction of this disease into

Two Principal Species.

The first is congenital, rather an absence of formation than a wasting of the medullary substance when once developed. It has been described by several authors, and particularly by M. CAZAUVIELH (who has published an excellent memoir on the subject in the *Archives Generales*, t. 14), under the name of Agenesia, cerebral agenesy. The other form of the disease is not congenital; it occurs at a certain period after birth; it does not depend on original absence of the part, but is an acquired disease, produced by causes that we shall presently have occasion to investigate.

General Anatomical Characters.

Atrophy of the nervous centres, gentlemen, whether it be an acquired or a congenital malformation, may present itself to our observation under a great number of forms and degrees, from the slightest diminution in the normal volume, either of the totality or any one portion of the cerebro-spinal axis, up to the complete absence of all those parts together,—of the brain, the medulla oblongata, and its prolongation into the cavity of the vertebral canal. Between these two extreme cases we find an immense number of intermediate degrees, to the principal of which we shall presently recur in detail. No matter what may be the seat or degree of this atrophy of the nervous centres, the disease is recognised by a certain assemblage of anatomical characters, which we now proceed to lay before you. The gray substance enveloping the medullary pulp is more pale than natural. It very frequently retains its normal degree of consistence; but in some cases we find that the nervous substance has acquired an increased degree of density and consistence, at the same time that it has diminished in volume. The brain then appears more solid than usual; it has a harder pulp, and this is particularly the case when the atrophy attacks the convolutions or external layer of the cerebral mass.

The parts immediately surrounding the atrophied portions of the cerebro-spinal axis present sometimes certain lesions that we should not neglect to notice. These may affect either the membranes which envelop the brain and spinal marrow, or the osseous cavities enclosing the central mass of the nervous system.

When the Diminution of the Brain or Spinal Chord is very considerable,

the membranes take on an increased action, and throw out a quantity of serum in proportion to the loss of substance which the nervous centres have suffered. In these cases we find a large quantity of serous fluid effused into the general cavity of the cranium, or into the ventricles of the brain and between its different convolutions, occupying and filling up the void that has been

left by the atrophied brain; however, when the bones of the cranium, as is sometimes the case, present an equal degree of atrophy, when the skull, if we may use the expression, follows the retreat of the brain, and is applied closely on its surface, then the quantity of serum is much less considerable, and the membranes display a degree of activity in harmony with the new object which they are called on to fulfil. It is not now the time to discuss where this serum is collected, or from what source it is more particularly derived. This question would lead us too far from the subject of our present study; however, we may remark, that it would appear, from the researches which have been made by M. Magendie, and at a still later period by M. le Docteur Cavara, that it is accumulated in the cellular tissue which is interposed beneath the arachnoid membrane. In some cases the serum is not diffused throughout the surface of the brain, but is collected together in masses, contained in one or more cysts, which supply the place of the absent nervous substance.

The Osseous Envelopes,

which protect the cerebro-spinal axis, may also present various changes, corresponding with the lesions of the soft parts beneath. Sometimes the osseous parietes are intact; the form of the skull is perfectly normal, and in this case the void left by the atrophied nervous matter is supplied by an effusion of serum, which we spoke of a short time back. When the disproportion between the cranium and the cerebral organ is great, the serum is found in large quantities; in other cases, the deficiency of nervous matter is made up by peculiar changes in the organization of the membranes themselves; the meninges are found much thicker than usual; they are incrusted with a cartilaginous or even an osseous layer, more or less thick, according to the degree of atrophy of the brain.

You may remark the analogy existing between this latter circumstance, and what takes place in other parts of the body. Thus, when a portion of the lung has been destroyed or atrophied, a lesion that frequently exists at the summit, in consequence of abscess, induration, and various other accidents, – when this, I say, occurs, and, at the same time, the thoracic parietes are unable to accommodate themselves to the altered form of the lung, to follow its atrophy by a corresponding degree of contraction, we frequently find osseous or cartilaginous matter thrown out between the ribs and surface of the lung, nature thus endeavouring to fulfil, by a morbid process, an indication which the state of the respective parts rendered impossible by a normal process.

We have now alluded only to cases of atrophy in which the parietes of the skull did not present any alteration of form or bulk, but this is not always the case; the osseous parietes of the cerebro spinal axis more frequently indicate, by a vicious state of conformation, the different degrees of atrophy that exist in the brain. In some cases the bones are thicker than in the normal condition, and yet this increased thickness of the skull is not appreciable on external examination, because the inner table alone is the seat of an increased nutrition. Here we have no default of development in the bones, corresponding with the want of development in the brain; but, in other cases, this is more marked. Thus, in a certain number of examples, the brain is more or less atrophied, and at the same time the osseous parietes sink down, and, as it were, follow the nervous substance in its retreat. The diminution of the cranium now alluded to may be partial or general, when the atrophy of the brain is general and complete; when the want of development in the osseous parietes of the skull is carried to a very great degree, and is accompanied by a general absence of the brain; in a word, when we find no trace of nervous pulp where the brain should be, the malformation has received the name of anencephaly; this is the highest degree of cerebral atrophy. The volume of the head is reduced to the smallest dimensions, or, rather, no remnant of the cranium exists to show that its development had ever begun. However, we cannot now occupy ourselves with this lesion, which belongs more properly to the history of monstrosities.

In a certain number of cases, where the volume of the brain has been considerably diminished, or even the organ is completely wanting, the cranium, instead of following the diminution of the cerebral matter, retains its ordinary dimensions, or acquires an unusual volume, and becomes developed much beyond the normal standard. Here the bones are hypertrophied; they are more solid, more massive, more thick, than is natural; or, on the contrary, preserving their soft and membranous condition, they yield easily to the distending force of the serum which is shed between the membranes of the brain, and accommodate themselves to the increased bulk of the matters contained within their cavity. In this condition, the brain, and its osseous parietes, frequently attain a great volume, and are designated by the name of

Chronic Hydrocephalus.

We do not mean to assert, in an exclusive manner, that, in all cases of chronic hydrocephalus, the absence of the brain, and the development in the cranial case which accompanies this absence of the nervous mass, is always an effect of some original malformation in the brain; it may, and in fact does, sometimes depend on various diseases of the membranous envelope, as

inflammation, increased secreting activity, &c.; however, in some cases, the atrophy of the nervous substance, with increased bulk of the head, does really depend on an original error of nutrition.

Finally, in some cases that are not very unfrequent, the membranous parietes which supply the place of a bony envelope, open; the cerebral substance, unsupported by its usual protecting boundaries, escapes from the cavity, and forms a

Cerebral Hernia

externally; or a membranous pouch filled with serum, and bearing more or less resemblance to a cyst, projects through the deficiency in the cranium.

In the few observations which we have now laid before you, we have not attempted to give anything like a complete history of the different lesions observed in cases of anencephaly, of acephaly, or other malformations of the cerebral mass connected with atrophy. Our object has been merely to touch in a general manner on the relation which exists between these different states, and a default of development in the brain and its osseous parietes. They were also necessary to explain the numerous and considerable lesions which we sometimes observe in the parts, whether membranous or bony, that surround an imperfectly developed brain. Let us now turn to a consideration of the

Causes of Atrophy of the Nervous Centres.

These are not always the same. Some of them depend upon the non-accomplishment of certain acts or formations that should have taken place during intra-uterine existence.

One of the most beautiful results of the researches which have been made within latter years to determine the mode of formation of animal bodies is the discovery of this curious law, "that most of our organs pass through a series of successive revolutions, each of which represents a normal and permanent organization in inferior animals." The operation of this law is seen in the brain as well as in other organs. Hence at any period when the centre of the nervous system is not yet completely formed, and an arrest of development takes place, implicating either the whole organ or any particular portion of it, we may observe the different types of atrophy which we described a few moments ago, the lesion being more complete as the developmental nisus is arrested at a more early period, and in different parts of the brain at the same time. Here then is the first cause of atrophy of the nervous centres; it consists simply in a modification of nutrition, in an arrest of the "nisus formativus;" but why this arrest should take place at one period of fetal life rather than another; why it should attack one portion of the nervous centres, leaving others that are contemporaneously developed, intact; these are questions that we are unable to resolve. We can only indicate the arrest of development, without being able to mount beyond it. It is worthy of remark that in fetuses born with more or less atrophy of the nervous centres, we frequently observe at the same time a deficiency of several other organs, showing that the lesion of the brain really depended in an arrest of its development; thus the heart is formed in imitation of the batrachia; the lungs are absent, and the digestive tube presents more or less resemblance to the simple alimentary canal of lower animals.

Again, imperfect development of the brain may depend on various maladies of the membranes which envelop the nervous substance, and by which, according to some physiologists, the nervous pulp itself is secreted. We can understand readily how this may take place, by supposing that the membranes draw towards themselves an unusual share of nutrition, or by a too great activity of secretion prevent the development of the more solid parts. This latter is a powerful cause of arrest of development in several organs contained in the cavities of the chest and abdomen, and may certainly operate also in modifying the nutrition of the cerebral mass. When the absence of a normal degree of development depends upon some disease of the brain, or on inflammation of the pia mater, or irritation of the cellular tissue that every where supports the pulp, the brain may present a general change of form rather than an absence of any considerable portion of its substance. Thus in the latter set of cases alluded to, where the cavity of the cranium contains an unusual quantity of serous secretion, all the different parts, whose ensemble constitute the cerebral hemispheres, have been found to exist. The hemispheres are as it were unfolded; the convolutions are separated from one another and spread out into a vast layer of medullary tissue, resembling a membrane, but on close examination, we find that the nervous substance remains intact in the midst of changes apparently so profound. There is a modification of form only, but no actual diminution, no atrophy of the mass. This has been frequently seen by Dr. CHASSAIGNAC, who has paid particular attention to the changes that take place in the arrangement of the nervous substance during the progress of chronic hydrocephalus.

There is another Species of Atrophy

of the nervous centres which we must not neglect to mention. Here the brain is very small, very small indeed, but it is normally constituted; all the parts exist in a state of perfect integrity. We find the hemispheres, the corpora striata, the central parts, such

as the septum lucidum, the soft commissure, and the fornix, the optic thalami, in a word, all the component portions of the brain, are found, but they exist as it were in miniature. The organ has suffered from a general arrest of development, acting equally upon all parts, and giving rise to an excessive smallness of the whole mass, without any malformation of a single part.

We have now touched upon a great order of causes which produce atrophy of the cerebral hemispheres, viz., disease or irritation of the membranes, and we are the more induced to admit the operation of this cause when we reflect that in a great number of cases the meninges are injected, thickened, contain abnormal deposits, and present more or less trace of antecedent disease.

Cerebral Atrophy from Accidental Productions in the Brain.

Certain atrophies of the nervous centres depend on a different order of causes. Thus suppose a tumour developed in the anterior of the skull: this must necessarily exercise a greater or less degree of mechanical compression; and the convolutions thus pressed upon, are sometimes completely removed, or at least reduced in bulk and volume. In other cases these accidental productions, instead of acting in a mechanical manner, seem to occasion atrophy of the cerebral substance by exciting a certain degree of irritation in the parts with which they are in contact. Hyperemia is the first result, and when once the nutritive *nisus* is deranged, a state of atrophy may succeed this deviation of nutrition, as well as a state of hypertrophy. This is a curious fact, though a certain one, that irritation gives rise to diminution of volume in the part which is the seat of excitement. We have had occasion to observe a remarkable case of this latter kind in the person of a man twenty eight years of age; who at the age of three fell on his head into the street from a first-story window. The accident was soon followed by paralysis on the left side of the body. The intellectual and other faculties, however, remained intact. This individual died of an inflammatory affection of the abdomen, and on examining the body after death we found that all the middle portions of both hemispheres situated above the lateral ventricles were completely gone; nothing remained of these parts except a vast cavity filled with limpid serum. Here the lesion supervened on external violence giving rise at the commencement to irritation, probably inflammation of the membranes, and subsequently to extensive atrophy of the hemispheres.

Finally, various diseases of the brain may determine an atrophy of the nervous substance immediately surrounding the affected part of the organ. Thus some observations seem to establish that the part of the brain which surrounds an apoplectic cell may in the end become atrophied. At least it is certain that in individuals who long before their death had presented all the symptoms of cerebral hemorrhage, a portion of the brain, and particularly the corpora striata and thalami (parts, by-the-by, not frequently subject to effusion), has often been found much diminished in volume, and transformed into a species of serous cavity.

We have now exposed at some length the anatomical characters of atrophy of the nervous centres. We have laid before you a history of the lesions observed in the osseous parietes, and in the membranes enclosing the brain. It remains for us only to add, that in some cases we are unable to discover any cause which has given rise to the diminution of volume in the brain. Here we may be permitted to ask, Is the default of nutrition connected with a want of due exercise of the intellectual faculties? A muscle condemned to constant inaction wastes away. May it not be the same for the organs of intelligence? This is not improbable.

The lesion which we have designated under the term "atrophy," does not present itself with the same frequency in all parts of the nervous centres. It becomes more rare as we pass from above downwards, from the surface of the brain to the more deep-seated parts; and it may be established as a general rule that it exists less frequently in those parts of the cerebro-spinal axis which are first developed, than in the portions which arrive more slowly at their perfect degree of organization. Thus atrophy of the spinal marrow is more rare than atrophy of the brain, and, again, in the cavity of the cranium, the deep-seated parts, which are first formed, are less frequently the seat of this lesion than the convolutions. We now propose to pass in review the various degrees of atrophy of the nervous centres according to the different points in which they have been specially observed. We shall follow their order of frequency, and trace their history, in the cerebral hemispheres; in the deep-seated parts; in the annular protuberance (*pons*); in the cerebellum; and, finally, in the spinal marrow. And first for

The Anatomy of Atrophy of the Cerebrum.

Here the lesion may present a great number of degrees, from a slight diminution of the normal volume, to the complete disappearance of the organ Let us commence with the most severe cases. A certain number of examples have been recorded in which the whole mass of the cerebrum was entirely wanting: there was no brain whatever: and in these cases it is at the same time proved that extra-uterine existence is impossible; or at most possible only for a few short days. The unfortu-

nate beings subject to this degree of malformation, seem made to live only in their mother's womb, and as they are rendered incapable, by their organization, of thinking or feeling, or performing at a future time any single act of animal life, so they are incapable of an independent existence, and after a few hours of vegetation lay aside life, which is incompatible with the absence of so large a portion of the nervous centres. This is the highest degree of atrophy of the brain. In place of that organ we find nothing but a slight trace of medullary lamellæ, which afterwards form the brain in a normal condition; or the nervous substance is supplied by a secretion of serous fluid, or by a mass of vasculo-cellular tissue, the rudiment of that membrane which is destined to surround and support the different parts of the interior of the brain. We have already spoken of the condition of the bones in this state; sometimes they are flattened down upon the base of the cranium; sometimes we do not find any trace of conformation of a skull; the parietes are membranous, and contain here and there irregular spots of ossification.

In other cases, as we have already mentioned, the destruction or absence of the cerebral mass is not indicated by any external symptoms. Thus Beclard has cited the case of a child who lived five days; the cranium was well formed, but it contained no brain; the whole of the cerebrum and cerebellum was absent, and their place supplied by a great quantity of serous fluid contained in the membranes; the spinal marrow however was not absent; this portion of the cerebro-spinal axis had attained its normal degree of development, and terminated superiorly in two bodies corresponding with the cerebral peduncles (crura cerebri), each of which again terminated in a kind of *renflement*, or swelling of the nervous substance; two small chords were observed detached from these bulbs, which ran forwards towards the ethmoid bone, became engaged in the groove of the ethmoidal plate, and, finally, terminated in an oblong swelling, exactly analogous to the bulb of the olfactory nerves: here an arrest of development produced in man a disposition of the brain peculiar to certain animals, which, you know, are furnished with olfactory lobes placed in front of the cerebral ones, and destined solely for the production of a single pair of nerves, the olfactory.

The history of the symptoms connected with this form of cerebral atrophy is not long; indeed, it is unnecessary to enter on it. We have seen that this child is, from the instant of birth, condemned to a premature grave; it may enjoy an imperfect life for a few short days, but death is an inevitable consequence of this malformation.

Next, to complete absence of the whole cerebral mass, we may arrange cases where one or both hemispheres are wanting, and here also the individual is incapable of independent existence. We must distinguish these two cases from those where, in consequence of imperfect development of the superior masses of the hemispheres, we might be led to imagine that the brain was wanting altogether. In the form we now allude to, there is an absence only of the parts which are situated above the level of the ventricles; when the malformation or atrophy is thus confined, when the important parts comprised in the cerebral mass placed beneath the level of the lateral ventricles are sufficiently developed, then a great fact comes to light—viz., the possibility of extra-uterine existence. In

Atrophy of the Nervous Substance placed above the Level of the Lateral Ventricles,

we may observe two degrees. In one, the atrophy exists at one side of the brain alone; in the second, it has implicated both hemispheres at the same time. Sometimes the whole of this superior portion of the brain is replaced by a membranous bag containing serous fluid; at others, the lateral ventricles are laid bare, and without any dissection we see the thalami corpora striata &c. This case, as we have already observed, does not necessarily compromise life; the individual may continue to live for several years, but all his faculties and senses are obtuse, many of them annihilated, and he lives in a state of vegetation rather than of humanity. When the absence of the substance is less considerable, and confined to one side of the brain, life, as you may conceive, is carried on more, much more, perfectly. This degree of atrophy has often been observed in persons arrived at an adult age. It is compatible with a perfect integrity of organic life, but the intellect is generally obtuse, dull, or idiotic. However, cases of an opposite kind have been seen, and it is remarkable to observe how the intellect may sometimes be conserved with a very great absence of the substance of the brain. M. Breschet has published a curious case relative to a child, four years old, where the intellectual faculties were much altered; the infant was plunged in a complete state of idioc.; but what we notice this fact for is to point out the occurrence of mutism. The child was dumb, although not deaf. In all the other cases observed, we do not find mention made of a similar phenomenon. Again, in a great majority of cases we have hemiplegia: the power of motion is lost in one side of the body, although the nervous mass lying below the level of the lateral ventricles exists in a perfect state of integrity.

We now pass to an examination of those more severe cases where the substance of the brain below the ventricles is more or less atrophied. Here we have to consider the lesion as it may occur in the anterior lobe,

in the middle, or in the posterior lobe. We may remark that the anterior lobes are wanting more frequently than either of the two others, and this atrophy may affect either one of the anterior lobes alone or both together. When both anterior lobes are absent together, we generally find the cranium deformed at the same time: it is flattened down in the forward part, and the forehead is more or less gone. However, some cases of the kind we now speak of, may exist with an excellent conformation of the forehead. Here the cerebral mass which is secreting is supplied by a quantity of serous effusion, or by membranous cysts filled with a clear fluid. What are the symptoms that accompany this

Absence of the Anterior Cerebral Lobes?

How are the great functions affected? Is the intelligence modified? How far? What lesions of motility does it produce? These are questions that we must now endeavour to resolve.

The intellectual faculties are generally modified in a considerable manner, or sometimes completely abolished. Thus M. Breschet has recorded a case in the *Repertoire d'Anatomie*, of a girl, 15 years of age, in whom both anterior lobes of the brain were supplied by a membranous cyst, at the bottom of, and behind which, the corpora striata were seen distinctly exposed. This child was a complete idiot; she could not clothe herself, required to be fed, and remained constantly in the same position without taking any notice of what passed around her. What symptoms do we observe in connection with movement? In some cases motility does not undergo any modification, although both the anterior lobes of the cerebrum are simultaneously wanting. This took place for example in the person of the idiot child just mentioned: her listlessness and constant rest did not depend on paralysis or feebleness of the muscular power, but on an absence of volition. The same phenomenon, viz., absence of paralysis may also be seen in two other cases published by M. Breschet: in other cases we observe nothing but a general feebleness of the limbs, and this gradually increases until the patient at length falls into a state of paralysis, being unable to move either his upper or lower extremities.

In one of M. Breschet's cases there was an abolition of vision. The child, nineteen months old, was perfectly blind, yet we cannot see any connection between the anterior lobes of the brain and the faculty of vision. We cannot understand why an absence of these parts should bring with it complete cecity. The truth is, the more we see of diseases of the nervous system, the further we advance in the study of their history, the more knowledge we attain upon the complex point of medical science, and so much the more are we convinced of this fact, that the different acts of the brain are connected together as intimately and as closely as are its different anatomical parts by a continuity of organization; and so much the more do we feel the necessity of studying in the nervous system, first, its detailed acts, and secondly, its action of *ensemble*.

In another case of absence of the anterior lobes, we find loss of the sense of smelling: this phenomenon is readily explained, and need not arrest our attention. The olfactory nerves are attached to the anterior lobes. When the latter are wanting, it is quite natural to find an absence of the power of smelling. Now for the cases

Where only One Anterior Lobe is Absent,

the other presenting its normal degree of development. Here the brain does not seem to have any great influence on the intelligence; it is ordinarily developed in a moderate degree. The individuals, though not, perhaps, remarkable for talent &c, possess a common share of judgment, good sense, and knowledge of society. In some cases, however, the intellectual faculties are modified; the individual is far from being an idiot, but his mental power is evidently diminished; the intelligence is dull, slow, weaker than natural. In most of these cases of atrophy of a single lobe, we observe hemiplegia, and we draw the same deduction for these that we have done for cerebral hemorrhage, viz., that the paralysis almost constantly occupies the side of the body opposite the injured hemisphere. In one case, which is recorded in the fifth volume of our *Clinique*, p. 620, the patient, a man, seventy-one years of age, in addition to complete paralysis of the left side of the body, was affected with an extraordinary difficulty of speaking, or rather of articulating; he could only mutter a few unintelligible sounds. After death we found the anterior quarter of the left hemisphere replaced by a transparent cyst, containing a limpid serosity. This, you may remark, is in accordance with the observations of M. Bouillaud, who attributes the power of co-ordinating language to the anterior lobes of the cerebrum. Examples of atrophy, or even of complete

Absence, of the Middle Lobe,

below the ventricles, have also been observed. These cases were accompanied, like the former, with hemiplegia of the opposite side, and the intelligence presented the same modifications as those which we have just mentioned. The same remark applies to atrophy of

The Posterior Cerebral Lobe.

The symptoms do not differ in any point worth noticing from the former cases; we shall, therefore, content ourselves with sim-

ply mentioning the fact. In one case we observed an absence of the middle and posterior lobes of one hemisphere at the same time. It occurred in the person of a man twenty-eight years of age, whose case we have already spoken of at the commencement of the lecture. The middle and posterior lobes on the right side were completely gone; nothing remained but the membranes floating in a clear liquid, and a portion of the anterior lobe situate in front of the corpus striatum. Here was absence of a very considerable portion of the cerebral substance: yet, notwithstanding, the individual enjoyed a common share of intelligence; his memory was good; he spoke freely and easily. Up to the day of his death he never presented any symptoms of disorder of the mental faculties.

You are all acquainted with the position and form of a cavity which anatomists have called the

Posterior Horn of the Ventricles.

Dr. HASTINGS has described two cases of atrophy of this portion of the hemispheres: you will find them detailed in the 59th volume of the *Bibliotheque Médicale*. CUVIER had established, that this posterior horn of the lateral ventricles is very much developed in the brain of man, while it is much smaller, and sometimes quite a rudiment, in inferior animals. We are totally unacquainted with the part it plays in the manifestation of such or such faculty; we shall only remark, as connected with CUVIER's observation, that in the two cases alluded to, there existed a lesion of the grand characteristic which distinguishes man from animals,—intelligence.

In all the cases of cerebral atrophy which we have hitherto passed in review, certain parts of the hemispheres were absent; there was a real loss of substance: the brain did not present the *ensemble* of all those parts which constitute it in the normal state. But atrophy may present itself under another point of view. Here we do not find an actual absence of any part of the brain; there is a general

Diminution in the number of the Molecules of the Brain.

The brain is well formed, but much smaller than in the normal state. This diminution of volume, affecting the cerebral substance without changing its organization, may occupy a single hemisphere alone, a single lobe, a portion of a lobe, or exist on both sides at the same time. An example of this latter kind, published by Dr. CALMEIL, is, perhaps, the most remarkable we have on record. (See *Journal Hebdomadaire de Médecine*, t. 1, p. 225.) All the parts composing the cerebral hemispheres were imperfectly developed, and equally so on both sides, and the patient was epileptic. No other lesion of movement or intelligence existed. This is a fact worthy of your attention; it cannot fail recalling to your minds what we said in an early part of our course, where we showed that a variety of lesions often coexist with the same symptom, when the nervous system is in question. Thus in our last lecture we noticed a case of epilepsy produced by induration of the cerebral substance. Here you see the same symptom manifesting itself as a consequence of atrophy; the organic lesions differ widely from one another; the functional symptom to which they give rise is the same. The form of atrophy of which we speak, may be circumscribed by a few convolutions; in that case we often find them hard and compact, and wrinkled on the surface like a cicatrix. Instead of the convolutions, the atrophy may occupy the more deep-seated parts, viz., the optic thalami, and the corpora striata. These two elevations may be either simply diminished in volume, without any change of structure, or the atrophy may engage more particularly the gray or the white substance which enters into their composition. In some cases we find not a diminution, but a complete

Absence of the Thalami and Corpora Striata.

They are replaced by a serous cyst, or we may simply observe their absence without finding anything in their place. When these bodies are simply diminished in volume, the hemispheres may or may not be wanting; but when the thalami and striated bodies are absent, we constantly find at the same time an absence of all the nervous substance placed above them. We find nothing but a membranous tissue, and a few scattered filaments. The brain represents the condition in which it existed at a very early period of fetal life; it represents the normal type of the cerebellum in a certain class of organized beings.

Atrophy of these parts, whether general or occupying only one side of the brain, has been observed at all

Periods of Life,

from infancy to the most advanced old age. Sometimes the cranium is depressed upon the portion of the brain seated beneath the absent parts. Here the lesion is discoverable during life, and we have a state exactly opposite to that form of hypertrophy where the osseous parietes seem to be pushed forwards by the development of the brain.

The different modifications of intelligence that we have already pointed out, have existed in individuals affected with atrophy of the thalami and corpora striata, and whenever the diminution of volume has been at all extensive, we have paralysis of the opposite side of the body.

The White Central Parts

of the brain, as they are called, may present various examples of atrophy. They may be diminished in volume or be absent, without any lesion of the rest of the hemispheres. In a few cases atrophy of the septum lucidum has been observed. In others the want of development affects more particularly the corpus callosum, and this mass of cerebral matter is reduced to the form of a fine membrane. Reil has left us a remarkable example of this latter kind, in the person of an idiot, thirty years of age, in whom the corpus callosum was completely gone, and the two hemispheres of the brain merely united by the anterior and posterior commissures. This is another example where an arrest of development in man gives rise to a form of structure that is peculiar to animals. Thus, in birds and reptiles neither the septum lucidum nor the corpus callosum normally exists. You will also find some curious cases of atrophy of the central parts, detailed in the thesis of M. Boulanger, No. 195, for the year 1821. In two of these cases there was complete absence of the corpus callosum, and in one the septum lucidum also did not exist. The patients were both idiotic, and one was unable to walk, or sustain herself on her limbs.

Atrophy of the Pineal Gland.

Before we conclude, let us say a word on atrophy of another part, whose use is not yet established, although it exists constantly in the four classes of vertebrated animals. The pineal gland presents a great number of variations in volume: in some cases it is excessively small, not larger than a grain of millet-seed. In others it is so far reduced, that we may in effect consider it as wanting. These varieties in the bulk of the pineal gland do not bring with them necessarily any modification of development in the other parts of the brain, and we are not aware that they are attended with any appreciable symptom. Some writers, indeed, have attempted to connect atrophy of the pineal gland with a diminution of the intellectual faculty, but this is a theory unsupported by a single fact, a supposition in which we can place no belief whatever. We shall continue this subject in our next lecture.

NORTH-LONDON HOSPITAL.

CLINICAL LECTURES

ON CASES OF

DISEASE OF THE URINARY ORGANS.

Delivered in the Session of 1836,

BY MR. LISTON.

LECTURE II.

(Concluded.)

Catheterism in Cases of Stricture.

Now I would have you follow, in these cases of bad stricture, the practice I have detailed, such practice as, in fact, we have pursued in the several cases I have brought in review before you. You should try to get an instrument into the bladder. It is all the same whether it be large or small. Tie it in; and it is better to begin with a metal catheter, such as this, at once, than to be poking away with these wax catheters, elastic bougies, and bits of fiddle-string. What can you expect by trying to put such a thing as that (it is called a catgut bougie, I think) into the bladder? It is better to work with an instrument with which you are sure to succeed. You are recommended by Sir Benjamin Brodie, a great authority in these matters, in this book of his, to try first all sorts of things, gum catheters, catgut bougies, et cetera; then you are to bend your bougie after this, that, and the other fashions and if you fail notwithstanding, you are at last to have recourse to the silver catheter. Why, the mischief, should you not take the silver catheter at once and pass it into the bladder?

Well, you introduce the catheter, and how does it act? Why, in the first place it is grasped and held firmly by the stricture, so that the patient or the surgeon would find very great difficulty in withdrawing it. But after a little time the parts become impatient at the presence of this foreign body, and a natural process is set up for its discharge. It is exactly the same process as that which takes place to favour the escape or ejection of a foreign body lodged in any part of the living tissues. There is a profuse discharge from the surface of the urethra, and the parts all become widened and relaxed; and unless the catheter were tied in properly, it would tumble out. In the course of twenty-four hours, part of the water comes to be discharged by the side of the tube. The next day you find the instrument still looser, and the discharge more profuse. You then cut the tapes with which the catheter is tied, and can pass a larger one without any difficulty. Then allow the

parts to rest for a little. I should say, that in two days it would be well for you to introduce an instrument again, to see that every thing is right, and then make the intervals of introducing the instrument much longer. Pass one at the end of four or five days, and thus proceed until you have brought the canal to its natural state. I would not have you retain these catheters longer than forty-eight hours, or three days at all events, otherwise a great deal of mischief will arise. You find, if it is kept in very long, that the further end of it, the eyes, become coated over with earthy matter, and, in withdrawing it, portions of that earthy matter have sometimes been separated, and a nucleus for a calculus has been formed in the bladder. Another mischief which I have seen to result occasionally, is the formation of abscess in the canal, and, frequently, anteriorly to the scrotum. A false passage is the consequence, and it is very difficult to get an opening in this part of the canal to heal up. It has been proposed in what are termed "impassable strictures,"—but there are no strictures impassable, that I have seen, for *when any water comes away*, you can, by patience and perseverance, get a catheter through, sooner or later,—it has been proposed to introduce either an elastic gum catheter, as recommended by DUPUYTREN, or a silver catheter, down to the stricture, and to keep it pressed against it, in contact with the obstacle; and it is said that in general, after a little time, the catheter will slip of itself into the bladder. I have seen this tried, but I cannot say that I ever witnessed any good result from it. I should think, that if a catheter will slip through in this sort of way, the surgeon would be able without much difficulty to introduce it. Here are two of the last works on this subject, one by a St. George's Hospital, the other by a Westminster Hospital surgeon. I have derived from them a great deal of satisfaction. There is a case related in the work of the latter gentleman which has amused me not a little. The method of securing the catheter is detailed, but I would recommend you to follow the mode you see adopted in this hospital, merely fastening a tape to each of the rings of the catheter, bringing them under the thighs, and then tying them to a handkerchief round the waist. There is no necessity for bandaging up the penis, and putting rings on it, and tying it up in lint and bobbin, as is sometimes done. In the case related in the book, an instrument, a gum elastic catheter, was put down to, and kept in contact with, the stricture. The patient was kept very quiet, and "the catheter kept going a little further in," for eighteen days, without its being passed through the stricture, though all that time the patient was making water by the side of the instrument. What it was pressed into or against all this while I cannot pretend to say, but such is the happy commencement of the practice. After a time they did succeed in getting an instrument into the bladder, and after that, in order to make up for lost time, I presume, they went on passing instruments every day, a practice which I have already reprobated as producing great inconvenience and suffering to the patient, and as not exactly the way to restore the parts to their natural state,—certainly not the way to tranquillize them; although Sir EVERARD HOME, after the four hundred and eighty-nine applications of his caustics, thought it necessary that the patient should have a bougie put in to keep the parts quiet and tranquil.

Cases of Stricture requiring the Employment of the Knife.

Now, sometimes you will not succeed in passing a catheter into the bladder, and you cannot expect to succeed where the passage is completely closed. Cases of that kind are now and then met with,—cases resulting from injury, or from violent inflammatory action, followed by the formation of matter, loss of substance, sloughing, and so on, in the perineum. In such cases it may be necessary to cut down into the passage, and to carry the knife forwards into the stricture, until you meet with the catheter passed down the sound part of the canal from before. This can only be required where the water flows through fistulous tracts, and none whatever is passed through the natural canal for a considerable time. I have, however, already stated, and proved by cases, that you may sometimes succeed in introducing a catheter when no water has passed for some time. I succeeded in one case where the patient had made no water by the urethra for fifteen months; but where the passage is completely obliterated, where it is closed, and closed from violent inflammatory action, (the result, say, of injury) while the cellular tissue all around is glued up, and there is, perhaps, an extensive deposit and organization of new matter,—in such case you must resort to cutting instruments. I have been obliged to have recourse to these operations in a few cases. One was that of a boy, about fifteen years of age, who had had his urethra cut across. He was engaged in the Carron foundry, and being about some mischief or other, was pursued by one of the workmen, when he jumped out of a window, and alighted on a bar of iron, which stood upright in the ground, in consequence of which he received a wound of the perineum, and had the urethra, it would appear, cut clean across. He was brought to me a long time afterwards, passing his water entirely through openings in the perineum. As there was no possibility of passing an instrument, however small, it became necessary to make an

incision into the raphè, to open the passage, so as to expose the end of the catheter, and to lodge the instrument in the bladder, for a time. I had occasion to pursue this plan, also, in the case of a very dreadful stricture, followed by abscess of the perineum,—a stricture resulting from a bruise of the perineum, in consequence of the man's falling from a great height on a beam of wood many years previously. All attempts to reach the bladder failed; the water was passed with difficulty, and with great straining, altogether, as it has been for many months, through various openings in the perineum. The urine was mixed with muco-purulent stuff. There was reason to suspect disease of the kidneys, in addition to the abnormal state of the bladder and its outlet. Under these circumstances, incision was resorted to for the re-establishment of the passage. In another patient, also, who positively declared he had not made any water through his urethra for about fifteen years, incision was had recourse to. He had been under treatment for stricture, had come to this city, and had had a caustic bougie introduced, rather roughly he thought (by no means an unfrequent source of complaint). This was followed by profuse hemorrhage. On riding down to Woolwich immediately thereafter, his horse shyed, and in addition to the injury inflicted by the surgeon inside of the passage, he got a severe bruise outwardly by striking his perineum on the pommel of the saddle. In consequence of these injuries, abscess and sloughing took place, and, in the end, the whole of the water was passed through the openings in the perineum I have mentioned. I am astonished that he should have submitted to this so long, for he was in the prime of life. I should think that he was not much over fifty years of age when he applied to me. When I first saw him, he passed his water through the perineal openings with difficulty, frequently, in small quantities. It was mixed with much ropy mucus, furnished by the thickened congested lining membrane of the bladder, no doubt. The bladder must have been very much diminished in capacity, the muscular coat being also thickened. He had great and frequent straining to make water. It was necessary to enlarge this artificial opening, the patient being secured in the position for lithotomy. I passed a catheter down into the perineum, near to the sinus of the urethra; not quite so far, but about four inches down; and on making an incision into the perineum, I cut forwards upon the point of the catheter, and readily carried it on to the bladder; the result was, that the whole of his water came through the natural passage, and he enjoyed himself after a short time, I suppose, just as much as he did before his mishap.

Now in making these incisions, you are not to cut, as in lithotomy, into the perineum, but directly in the mesial line, into the urethra, in fact; and in general there is no great difficulty in catching the posterior part of the canal. If you cut upon an impassable stricture, you know that the canal is greatly enlarged behind; and if you cut in the middle line, from near the apex of the prostate, the finger being placed in the bowel as a guide, directing the point of your knife upwards and forwards, you must hit the passage, and you are not interfered with by vessels. There is no hemorrhage; very little, at any rate. You can make an extensive incision, from the verge of the anus, into the perineum, and there is no difficulty in cutting into the anterior part of the passage. It is only in those cases where no water comes through the passage at all that you are warranted in cutting down, for, in general, by proper study of the parts, and by considering well the nature of the disease, and practising on all occasions the introduction of the silver catheter, you will attain such dexterity in its use, that you will have small difficulty, in almost every instance, whatever the case may be, in getting an instrument into the bladder.

Rupture of the Bladder from Retention.

In retention of urine there are very considerable peculiarities as regards the state of the bladder and the state of the urethra. In strictures the bladder is generally diminished in capacity, as I have before said. It contains but a very small quantity of urine; it can bear distention to a very slight extent, without occasioning unpleasant or uneasy feelings, and without great risk occurring if the viscus be not relieved; but this is not the case in all instances of retention. In some, the bladder is capable of containing two or three pints of water, and it rises up into the cavity of the abdomen exactly as the gravid uterus does, forming a large oval tumour in the "lower part of the stomach," as the ladies and their doctors call it. The belly is swelled, if I may use that phrase. There is a large oval swelling in the lower part of the abdomen. Now in some of those cases there is a much greater chance of the parts giving way, and an infiltration of urine taking place into the cellular tissue, than there is in others. In retention from stricture, more especially where an abscess has been in progress beforehand, there is a greater chance of the cavity of the abscess communicating with the urethra, the parietes of the urethra bursting, and the urine escaping into the cellular tissue, than of the urethra behind the stricture yielding. The risk in such cases is very great indeed, but the risk of infiltration from retention of urine is greater. In retention from obstruction at the neck of the bladder, from enlargement of the prostate gland, impactment of stone, or other cause, the urine is extravasated,

though rarely, and it is effused into a part much more dangerous to the patient. It is poured out, in consequence of the sloughing of the bladder, into the pelvic cavity. When that takes place, there is little or no chance of the patient's recovering, whereas when it arises from the giving way of the posterior part of the urethra, though the danger is great, yet recoveries often are made, even after the loss of much integument and cellular tissue, the covering of the testicles &c. In cases of

Retention from Injury of the Perineum,

you will in the first instance endeavour to pass an instrument into the bladder, so as to empty it for a time, until the parts have recovered in some measure from the effects of the injury. At a later period, when there is great swelling and some effusion of blood, more especially if the patient have strained to make water, you will have to make a free incision into the perineum, in order to allow the escape of the blood and water, mixed up together. At the same time it is sometimes though rarely necessary to pass an instrument through the lacerated urethra into the bladder. You may in some of these cases be under the necessity of puncturing the bladder. This has been called one of the regular operations of surgery, but it is such an operation as you will very seldom see performed here. Where there is no previous contraction, but where the retention arises from inflammation of the parts, you will be able to relieve the patient, without the employment of instruments at all, by warm-baths, and the exhibition of opium, with the addition of an opiate suppository. If the patient be not thus relieved, you must at all risks introduce a catheter into the bladder, and if there have been no previous contraction, there should be no difficulty in effecting your object. Again, in

Retention from Stricture,

which is the most difficult to manage of all, there is no faith to be put in opium, the tincture of muriated iron, or anything of that kind. There is then no time to be lost. Have recourse to the catheter at once. The parts are altered, and there is very great probability of the urethra suddenly giving way, not *bursting*, as people say in books, but sloughing, and of extravasation of urine loaded with saline matter, taking place into the cellular tissue. It is not very common for patients to recover from this, though they do recover now and then. You must introduce the catheter and retain it, because you may find a difficulty, if you withdraw the catheter, in getting it replaced, for the patient may be subject again to retention, so that you will be under the necessity of introducing and re-introducing the catheter; but if you do away with the chance of retention, keeping the instrument in, you make a great advance towards the cure of the stricture. There is a case related in one of the hospital journals in which this practice was pursued,—a case of very bad stricture, occurring in a patient, I think, of Mr. Cooper. He did not see the patient, as I think he was then out of town, but I happened to come to the hospital at the time, and found that the house surgeon had been foiled in passing the catheter. The patient, it is here stated, had been affected by gonorrhœa three years previous to this attack, and he had never been altogether free from discharge. He more than once had laboured under retention, from which he was easily relieved. However, the stricture had been getting worse and worse, and on this occasion, before his admission, his surgeon had attempted to pass an instrument, without success, and he then sent him here. He had passed no water for twenty-four hours; he had pain and tenderness in the perineum, very likely increased by the attempts which had been made to overcome the stricture. He had been put into the hot-bath, and long and continued attempts had been made to pass the catheter after he had come out of it, but without success. A false passage, it is said in this report, was discovered. He had forty drops of the muriate of morphia given to him, and the following is the report of the house surgeon:—"On Mr. Liston's coming into the hospital, he was asked to introduce the instrument, and by dint of considerable force,"—that I deny,—"and dexterity, he succeeded in passing the catheter." I did not use what may be called "force," though I dare say some little dexterity was required, and after the point of the instrument was lodged in the stricture, which you know by its being held firmly, steady pressure was perseveringly employed. The catheter being passed into the bladder, that viscus was emptied. The instrument was quite loose in two days. On the 6th, two days after it had been introduced, it was withdrawn, and he then made water with but little smarting. On the 14th he was discharged quite cured.

Now when abscess forms in the perineum, more especially

When Extravasation of Urine takes place,

you are called upon immediately to make a free opening into the perineum. Make an opening directly into the collection of matter and urine. As soon as this is done, the necessity for passing an instrument into the bladder has ceased, because the bladder empties itself into the perineum, and the water comes freely through the opening. Besides, if the urine be infiltrated into the cellular tissue of the scrotum or penis, you must make many good free punctures, of about half an inch in length each. It is time enough to pass the cathe-

ter into the bladder after all the irritation has gone off, after the swelling has subsided, and the sloughs have come away.

I have said, that in almost every case it is possible to succeed in introducing the catheter; but it *may* happen that you are *not* able to do so. You may then be under the necessity, as a last resource, of

Puncturing the Bladder.

I have already stated, that you will seldom see this operation performed here. I have had a pretty extensive practice in these cases both in public and in private, but I have never punctured the bladder yet, and I hope I never shall. You will find the same opinion entertained by other surgeons. Mr. THOMAS BLIZARD, under whom I studied, and no man could be more dexterous in the use of instruments, was decidedly of this opinion. You will find it stated in DESAULT'S works, that he once punctured the bladder, but that was when he had had no great experience in these matters, and when he first took charge of the *Hotel Dieu*; and he expresses his conviction, that if the case had occurred later in his practice, he should have succeeded with the catheter. But if you cannot manage to give succour by the catheter, why then you must puncture the bladder. You must relieve the patient from his sufferings at all hazards, and it would be better to puncture in all the three places at once—above the pubis, in the perineum, and through the rectum—than to allow the patient to suffer from extravasation and destruction of the cellular tissue.

There is only one other species of retention of urine, and that is, from disease at the neck of the viscus, and paralysis of the muscular apparatus of the bladder—from

Enlargement of the Prostate.

In general, in such cases, you can succeed in relieving the patient by the employment of a proper instrument—by a long catheter, such as this,—one at least four or five inches longer than those usually employed, the curve being large and the beak long. There is no use in poking away with shorter catheters, because you cannot by any possibility reach the bladder with them. The urethra is elongated by the enlargement, by the hypertrophied state of the gland; the prostatic portion of the urethra is immensely lengthened; and, besides that, the bladder is generally capacious; it is enlarged to an immense extent; after it has become so far distended and bulged for a time into the hollow of the sacrum, the fundus of it begins to yield, and then the viscus rises into the cavity of the abdomen, and in that way the cavity of the bladder is further removed from you than in the natural state; you require, therefore, a much longer instrument to reach it. I have seen many cases where it was supposed that the bladder was filled with blood, in consequence of no water being brought away by the catheter; and there have been lying about the patient's room instruments of all kinds,—catgut bougies, elastic gum catheters, and silver catheters, all bloody. The instruments have come out filled with blood, but that has been from laceration at the neck of the bladder. On introducing such a catheter as this, however, the water has come away naturally; not perfectly transparent, because it is mixed up with mucus of a dark colour. It contains a great quantity of saline matter, and it is in consequence of this change in the quality of the water, when it has been long retained, that such rapid mischief arises on its being discharged into the cellular tissue.

Now there is another cause of retention, and that is, the

Loss of Power in the Bladder

to expel the urine. We had a case showing this very well the other day. The retention was supposed to arise from stricture of the urethra, but my opinion, from the first, was, that it did not arise from that cause. The patient stated, that for a long time he had difficulty in making water, and he attributed it to his having been prevented from time to time from emptying his bladder. He was in the habit of driving out parties of ladies into the country, and he could not very decently get off the box to empty his bladder. Upon several of these occasions he had retained his water much longer than he otherwise would have done, and at last, when he came to pass his water, he found that he did so with difficulty. On this occasion it stopped entirely, and he applied to a surgeon, who introduced a catheter, but did not succeed in getting it into the bladder. When I saw him, the glans penis was swelled considerably, the whole organ was in an excited state, and I should not wonder—he was a man pretty well advanced in life—if he had been straining himself, or over-exerting himself in some venereal encounter, which it is not very safe in old men to attempt to any great extent. However, in consequence, the parts had become inflamed. He at first passed his water pretty well, although, perhaps, he could not empty his bladder entirely, but at last the water stopped altogether. An attempt was made to pass a catheter, as I have already stated, but without success. When the patient was admitted, Mr. WALLIS, my house-surgeon, with some little difficulty, succeeded in getting a small catheter into his bladder, and drew the water off. The catheter was secured in the bladder, under the impression that this was an organic and unyielding stricture, but the patient in his restlessness withdrew it, after it had been in for three or four hours. The consequence was that the urine reaccumu-

lated, and on my making the usual visit I found his bladder immensely distended, the parts very much swollen, the glans red, and some discharge coming from the parts. There was no gonorrhœa, but the parts were in a state of excitement, and a slight inflammation of the lining membrane of the passage existed, probably from the cause alluded to, and from the use of the instruments.

I passed a catheter of a pretty large size into the bladder without any difficulty, and I found that there was no such resistance, or grasping, as is met with in cases of bad stricture. The catheter was retained, and in the course of two days a much larger instrument was passed without difficulty. He says he has had symptoms of stricture for twenty years, especially if he caught cold, and from which he had some difficulty in making water. He has had very little surgical assistance until the retention became severe. He is of very temperate habits. On Sunday last he got wet from the rain, and on Monday he passed his water with difficulty. On Wednesday he applied to a medical man, who attempted to introduce a catheter, but without success, and at about seven or eight o'clock, he was brought to the hospital." Number 3 catheter was then passed in, to about an inch from the orifice, and it was supposed that he had a stricture, though I could not find any. Indeed, I think that if there had been, the passage would not have yielded quite so readily. "He was then put into a warm-bath. A little water followed the withdrawal of the catheter. Half a grain of muriate of morphia was then given to him, and fomentations were applied to the region of the bladder. When under the influence of the morphia, a small catheter was passed in, and a great quantity of water was drawn off. The catheter was tied in, but the man became restless, and it came out at 9 a.m. He passed a very good night. Number 4 catheter was put in on the next day, and four pints of water were drawn off."

Now my reason for thinking that there was no stricture, and that the disease was merely a want of power in the coats of the bladder, was that in stricture you very seldom find the bladder so capacious as it was in this case. It is generally contracted, in order to overcome the resistance arising from the stricture. There were four pints of water drawn off here, and in such cases the bladder frequently contains most likely not more than from four to twelve ounces.

I thought it was much more prudent, in this case, to retain the catheter in the bladder. In cases of over-distended bladder, where the canal is sound, the bladder recovers its tone better when the instrument is introduced only as occasion requires, I say twice or thrice in twenty-four hours; but here the difficulty, experienced by the house-surgeon in introducing the catheter, was a great objection to doing so, and it was thought best to detain the catheter, so that the bladder could be kept empty, and it would thus have an opportunity of recovering its tone. In a short time a full-sized catheter was introduced. The bladder was found capable of relieving itself in a full and free stream. The patient was accordingly dismissed cured, after but a very few days' residence in the hospital.

CASE IN WHICH

GOUTY CONCRETIONS

WERE EXTENSIVELY DEPOSITED IN

VARIOUS PARTS OF THE BODY.

To the Editor of THE LANCET.

SIR,—I beg to transmit the following particulars of an interesting case, which I obtained on seeing the invalid during a late visit in the country, for insertion in your valuable Journal. I am, Sir, your obedient servant,

T. HERBERT BARKER.
North-London Hospital, Feb. 11, 1836.

Mrs. C., ætat. 45, resides at Hemel Hempstead, Herts. She is emaciated, and of sallow complexion. She has never enjoyed very good health. She has had three children, all of which died either at birth, or in early infancy. She has never been accustomed to luxurious living. About fifteen years ago she had an attack of inflammation of the great-toe of the right foot, and of the carpo-phalangeal and phalangeal joints of the middle finger of the right hand, which lasted for some time, and left the parts a little red and swollen. She subsequently had *irregular* paroxysms, of a similar character, affecting the smaller joints of the hands and feet, and the ankles and heels. About seven years ago, after a severe paroxysm of heat, redness, pain, and swelling, in the joints first affected (those of the right middle finger), she perceived some "chalky matter" deposited in the parts affected, which, previous to its escape, very much aggravated the pain, by breaking the integuments immediately covering it. Since she first perceived the concretions, their formation has been so extensive, that there is scarcely a square inch of the surface of the fingers, the hands, the toes, or the feet, in which they may not be observed, particularly at the metacarpo and metatarso phalangeal, and the phalangeal joints, which are so extensively studded with protuberances of various sizes caused by the concretions, as to cause great deformity. She has had not less than a dozen openings in

the feet, and nearly the same number in the hands, from which great quantities of these concretions have escaped. Their formation has always been preceded by most excruciating pain, and the pain has always been relieved by the application of cold, and much aggravated by the application of heat.

Besides the existence of the disease in the parts which have been enumerated during the last six or seven years, the elbows, the shoulders, the knees, the hips, the *nose*, the *cheeks*, and the *gums*, have been the seats of similar pain, heat, redness, and swelling, followed by concretions in the right elbow and knee, and on the left side of the nose.

Her *grandmother* was a native of Dunstable, and was terribly afflicted with a similar disease. The *parents* of Mrs. C. were entirely free from the disease. They had never had an attack of gout in their lives. She has been repeatedly told by her friends that the disease was caused by her use of the water of Dunstable, because it was impregnated with saline matter, in consequence of the abundant calcareous strata in that part of the country.

She has been most egregiously neglected, never having had any medical attendance for her very painful malady. A long time ago, some friends recommended the application of Goulard's lotion, which she used once or twice, with temporary relief from the pain, but she desisted from its further employment, the danger of such applications being represented to her by some other friends as being considerable.

She says that nine or ten years ago she had a severe and long illness, with "brain fever," which was caused by insolation; and that, during the last two years, she has scarcely been free for an entire month from erysipelas of the head and face, or of the parts affected in the hands, where it has generally assumed a severe form.

At the present time, there is a protuberance of about the size of a horse-bean, situated just beneath the skin, opposite the inferior margin of the left nasal bone. It is hard and moveable, and the light-coloured concretion, similar to that which is so plentifully distributed over the hands and feet, is seen through the transparent cuticle covering it. The fingers are exceedingly deformed, there being clusters of indurated protuberances at almost all of the phalangeal and metacarpo-phalangeal joints; several masses of similar substance are situated upon the dorsa of the hands, and about the wrist. The white saline matter may be observed underneath the diaphanous epidermis, of various size and figure, in different situations. The mass, situated at the joint between the metacarpal bone and the first phalanx of the left index-finger, is of about the size of a hen's egg,—hot, red, and very painful, with a small but deep ulceration situated in the central and most prominent part of it, which has been there for four or five weeks, and discharges a considerable quantity of a rather thin and offensive purulent fluid mixed with the saline matter, and probably holding some portion of it in solution, imparting to the fluid a chalky-white colour. The metacarpo-phalangeal joint of the little-finger of the same hand is of about the size of a walnut, but is not red; neither is it, like most of the other protuberances, painful on pressure even, although it was the seat of excruciating pain at an earlier period of the affection.

The feet are similarly deformed, and the white masses may be seen, as in the hands, covering the toes, heels, sides, and dorsal surfaces.

There are three tubercles of similar concretion, the largest of about the size of a small pea, situated opposite the external condyle of the right humerus. And there is a small concretion deposited on the inner side of the right knee. The pulse is 78, and rather feeble; she has a slight cough, and expectoration in the mornings, in consequence of having caught cold a short time since; the skin is cool and dry, she never perspires excepting during the violent paroxysms of pain, when perspiration and cold shivering frequently come on alternately; and she generally has shiverings and flushes of heat alternate with each other for a short time previous to the attacks of erysipelatous inflammation. The tongue is dry, and covered with a whitish fur; the appetite has always been impaired; she is almost constantly distressed with urgent thirst; the bowels are generally somewhat relaxed; the urine is usually rather small in quantity, and high-coloured, depositing a heavy brown sediment; she has at no time had any difficulty in voiding it. The function of menstruation has usually been pretty regularly performed; at the present there is some little irregularity, which she attributes to the age at which she has arrived.

Remarks.—Similar cases to the above are not very frequently met with. The concretions usually occur only in one or two of the smaller joints, but here there is scarcely a small joint of the extremities that is not involved, together with one of the knees, one of the elbows, and the nose. The age of this poor woman, too, is rather early for their formation.

It presents an instance of this hereditary disease passing over a generation. Although the patient could not give me much information concerning the case of her grandmother, still she assured me that she was greatly afflicted with it. The parents of the patient lived to a good old age, and were completely free from it. This passing of hereditary affections over alternate

generations is not uncommon,* and here is an instance of it in gouty concretions. It is a popular notion amongst the inhabitants of places near the chalk hills (which are very numerous in Bedfordshire and some adjoining counties), that this disease is induced by the water of those parts, but we need not resort to such an explanation, since the chemical composition of these concretions, explains the pathological relation which subsists between gout and gravel. Moreover, it would be difficult to explain their peculiar composition from the imbibition of water from a calcareous soil. The only cases, however, which I have seen (three), and one of which I have heard, occurred in persons who belonged to such localities. Dr. Wollaston first demonstrated that the real composition of "chalk stones" was uric acid, combined with ammonia, potash, or soda, generally with the former. It is sometimes combined with lime, and phosphate of lime enters into the composition of some of them. I brought to town with me a few grains of the concretions which have escaped from the hands and feet of this woman; they are in light masses, of a white colour, soft, friable, and intersected abundantly with cellular tissue, insoluble in cold, but slightly soluble in boiling water. On adding to the solution a few drops of nitric acid, and evaporating it to dryness, the beautiful purple colour of the purpurate of ammonia made its appearance, formed by the decomposition of the uric acid, and highly characteristic of its presence. On placing a portion of the concretion upon platinum, and directing a flame by means of the blowpipe upon it, it gave out a strong animal odour, partly depending upon the cellular tissue entering into its formation. On continuing the flame it was apparently destroyed; not a particle remained, proving the absence of phosphate of lime; it did not entirely fly off, proving the whole of the alkaline base not to be ammonia. The surface immediately surrounding the spot on which the concretion was placed, was covered with a small quantity of a fused substance, which was soluble in warm water, and was proved to be distinctly alkaline. It is, then, very probable that the lithic acid in this case was in combination with soda or potash. The woman had not been in the habit of preserving the concretions after their escape, and the quantity that I obtained was so very small, that it was impossible to prove their exact chemical composition. An able chemist has kindly offered to make a quantitative analysis of these concretions when I shall have obtained a sufficient quantity; and probably at a future period I shall be able to publish more satisfactory information on this point.

* See Dr. Copland's invaluable "Dictionary of Practical Medicine," art. "Epilepsy," page 789.

TAR IN THE BLOOD.

To the Editor of THE LANCET.

SIR,—A short time ago, while experimenting on the human blood, I separated from it a peculiar species of petroleum; and on referring to the analysis given by several eminent chemists, I found that this principle was not at all noticed by them. I therefore am led to suppose that it has not before been separated from the blood, unless very recently, and published in some work which I have not had an opportunity of seeing.

Process.—One pound of blood was taken from a man about fifty years of age, and before separation took place, it was well mixed with one ounce, by weight, of concentrated sulphuric acid, and stood frequently for twenty-four hours. To this mixture was added two ounces of carbonate of lime, and the whole again was well stirred up till effervescence ceased. A glazed earthenware retort was then procured, having its beak lengthened with a tin tube, about three feet long, the extremity of which was introduced into a quart bottle, and the bulb of the retort into a small sand bath, which was placed in a charcoal furnace. A strong fire was gradually raised, while the beak was kept cold by means of wet cloths. Carbonic acid gas first came over, which was allowed to escape; then a most disagreeable smell was evolved, when it was found necessary to lute the receiver to the retort, to prevent its escape. At length a fluid distilled over, and with it the tar. Some floated on the surface, while some formed into globules and went to the bottom. When the distillation was complete, the tar was separated, by filling the bottle quite full of water, and taking the tar from off the surface by means of a slip of glass.

This is the process which I used for extracting tar from blood. For some time I thought it was an oil, and laid it aside as such; but the laws of chemistry do not allow us to judge of substances simply from their appearance, and further experiments proved to me that the product was decidedly tar, and not oil.

I afterwards repeated the experiment, by acting upon eight pounds of sheep's blood; and using the same proportions of acid and carbonate of lime to each pound. From this I certainly expected to obtain a large quantity of tar, but the retort not being glazed, a portion escaped through the pores. Consequently, I was unable to ascertain in what proportion it existed in the blood, but the quantity thus extracted was one fluid drachm.

Animal tar is adhesive to the touch, and lighter than water, and possesses about the same degree of consistency as common

vegetable tar, but it differs very materially from the latter in smell, for its odour is extremely disagreeable. It is soluble in alcohol, and turns of a thick brownish white colour when mixed well with water. By evaporating the spirit, the tar again floats on the surface. When ignited it burns like common tar, leaving a smell not unlike that of burnt feathers. The quantity obtained being too small for redistillation, I was obliged to heat a little in a bulb glass to the temperature of ebullition, when a spirit was given off which took fire when brought in contact with a lighted taper.

I hope that my experiments will be repeated, provided the result is yet otherwise unknown; not that I think it of great consequence, but we must always feel interested in seeing the secrets of nature unfolded. Transmitting this account for insertion in your excellent journal, I remain, Sir, your obedient humble servant,

HENRY OSBORN, Chemist.

Southampton, Feb. 16th.

BROWN BREAD.

To the Editor of THE LANCET.

SIR,—Allow me, through the medium of your valuable journal, to call the attention of the profession to the uses and effects of brown bread, both as a remedy in habitual costiveness, and also as an article of diet. I am induced to make these observations, in consequence of having myself experienced both its value as a remedy in the above-mentioned state of the bowels, and its injurious effects when used for any length of time as an article of diet. Its medicinal virtues are really surprising. It scarcely ever fails to remove, in a very short time, that habitual costiveness which so generally accompanies a sedentary life. But when its use is continued for some time after the costive state of the bowels has been removed, its effects are most injurious. It then produces all the symptoms of dyspepsia, and irritation of the whole alimentary canal. The laxative effect arises from the particles of bran which it contains, acting as a mechanical stimulant, and increasing the peristaltic action of the intestines. Now the very manner of its action as a laxative, proves its unfitness as an article of diet; as it is necessary for all substances, in order that they may have any mechanical effect upon the bowels, to possess the power of resisting the action of the gastric juice, and all substances that have this power (for instance the rind of vegetables, under which denomination bran comes) also possess the power of producing great derangement of the alimentary canal, and I believe I have, in some cases, seen inflammation of the mucous membrane produced by the continued use of bread containing bran. Brown bread is very extensively used in this city, and is sold under the name of "Constitutional bread," a name of which it is not deserving. I will at some future period furnish you with cases illustrative of its effects. Believe me, Sir, your obedient servant,

CHARLES WRIGHT.

Harcourt-street, Dublin,
Feb. 13, 1835.

SYPHILIS.

"A gentleman who had been cured of a chancre at a distance from home, called to consult me whether he might consider himself as perfectly free from the disease. Whilst he was taking great pains to explain to me how he had been salivated, and how long he had continued the use of mercury after the chancre was healed, I interrupted him by observing, that if he had continued the use of mercury till now, I could not pretend to say whether he was free from the disease. 'How then,' said the gentleman, 'am I to ascertain my real situation?' 'If,' replied I, 'you find no symptoms in the course of three months, the probability is that you will remain well till you expose yourself to a new source of infection.' In about six weeks he returned, with a sore throat and copper spots. I explained to him that he should not blame his surgeon, who, even if he had known what was to happen, could not have prevented it. The patient went through a necessary course of mercury, till he was cured of every symptom; and then demanded, with some impatience, whether he was secure. 'You are secure,' replied I, 'from every return on the genitals, and on your skin and throat; but, as it is impossible for me to know whether your bones are contaminated, I cannot pretend to say whether you will have nodes in a few weeks' time.' He now began to comprehend the doctrine, and submitted to await the result. In about six weeks he actually had nodes; after the cure of which, by a severe salivation, I made no scruple to assure him, he was perfectly free from the disease."—*Mr. Hunter's last conversation with Dr. Adams.*

The effects of the venereal poison upon the human body, Mr. Hunter describes under the three different states of "contamination," "disposition," and "action." By *contamination*, he implied the entrance of the poison into, and its contact with, some part of the body. By *disposition*, the intermediate state between contamination and the breaking out of the disease, or the state of *action*.

First, of the state of *contamination*.

Mr. Hunter was of opinion that mercury, if given immediately before, or upon, the appearance of chancre, prevents contamination. He says, 'If the poison were still capable of circulating after its visible effects were cured, mere mercury given in the time of a chancre can be of little service, as it can only assist in the cure of a chancre, but cannot preserve the constitution, which does not agree with experience; for practice informs us, *that not one in fifty would escape the lues venerea, if the chancre were only cured locally; so that mercury has the power of preventing a disposition from forming*, and therefore is necessary to be given, while we suppose absorption going on, or while there is matter that may be absorbed." —*Page* 429, *second edition, by Dr. Adams.*

Dr. Adams, commenting on this opinion, observes,—"This is a fair inference, but its truth can never be ascertained, because the primary diseased action, whether chancre or gonorrhœa, has always commenced before mercury is applied."—*Page* 433.

The experience of the military surgeons, especially of Mr. Guthrie and Mr. Rose, although of no very recent date, throws considerable light on this important point. Mr. Guthrie, in an interesting paper, in the *Medico-Chirurgical Transactions*, I believe, vol. viii, observes, — "During the last eighteen months, in the York Hospital, Chelsea, Mr. Dease, Dr. Arthur, Dr. Gordon, and myself, have been in the habit of treating all cases of ulcers on the penis, whatever form or appearance they might have, by simple mild means; that is, by dry lint, or ointment, or lotions, for the most part not containing mercury, in order to obviate the objections that might be made to the application of mercury in any form; and of near one hundred cases which have been treated in this manner, all the ulcers healed without the use of mercury. The primary sores were of every description, from the superficial ulcer of the prepuce and glans, to the raised ulcer of the prepuce, the excavated ulcer of the glans, and the irritable and sloughing ulcer of those parts.

"The secondary symptoms, in the cases alluded to, amounting to one-tenth of the whole, and which were treated on the antiphlogistic plan, have hitherto been nearly confined to the first order of parts—that is, the bones have in two cases only been attacked, and they have equally been cured without mercury."

"A gentleman, aged 27, contracted venereal sores in the month of June, 1833, and was put under a mercurial course, during which time the sores healed, but he still continued the mercury. While under the influence of that medicine, he indulged in impure connection, the consequence of which was a fresh crop of venereal sores, three or four. The mercurial course was prolonged until the healing of these second sores. About two months after this, secondary symptoms made their appearance, for which he was again salivated, with apparent success; but now a new enemy appeared in the field, in the shape of rheumatism, and he again applied for medical advice. The surgeon whom he consulted considering it a case of venereal rheumatism, arising from a portion of the poison still lurking in the system, advised a fourth mercurial course, but although attended with partial relief during the existence of the ptyalism, the rheumatism shortly after returned with redoubled violence, in which state the patient came under my observation."

The observation of Mr. Hunter, "that not one in fifty cases of chancre would escape the lues venerea, if cured locally," when placed in opposition with the experience of the military surgeons above stated, appears to prove one of two things, either that Mr. Hunter was mistaken, or that the poison has become much milder in its effects.

First—Was Mr. Hunter mistaken? Speaking of the acrimony of the poison, he observes, "Venereal matter must in all cases be the same; one quantity of matter cannot have a greater degree of poisonous quality than another; and if there be any difference, it is only in being more or less diluted, which produces no difference in its effects. One can however conceive, that it may be so far diluted as not to have the power of irritation. The variation of the symptoms in different persons depends upon the constitution and habit of the patient at the time. What happens in the inoculation of small-pox strengthens this opinion. Let the symptoms of the patient, from whom the matter is taken be good or bad, – let it be from one who has had many pustules, or from one who has had but few, let it be from the confluent or the distinct kind, applied in a large quantity or a small one, it produces always the same effect.

In a small pamphlet submitted to your notice, I have stated my conviction, that the poison or poisons producing gonorrhœa and chancre *do vary* in their acrimony, and are arising *de novo* daily, – that the majority of venereal sores are not true syphilis, or, in other words, are not followed by secondary symptoms, and, therefore, require for their cure little or no mercury.

Now, if we can throw off the idea of the venereal poison being like small-pox, a specific, uniform, and unvarying poison, and believe *that it varies in acrimony*, not only from the constitution, but from *the habits of life* of the patient, then nothing is more reasonable than the belief that *the effect will also vary in severity*.

It is well known that the disease is milder in France than in this country, which would appear in a great measure to be owing to

the necessity on the part of the females for cleanliness and personal ablution, in order that they may not be placed out of the "pale of health" by medical superintendence.

Mr. Hunter's comparison of the venereal poison with small-pox is, I think, inadmissible; since, while small-pox produces small-pox or nothing, three persons, having connection with one female, may have gonorrhœa, chancre, or bubo; and even these varieties may vary in acrimony. Now, while *the cause* which produces these diseases is one and the same, when once removed from the woman, it *does not necessarily* maintain its original character; the effect is not necessarily the same as the cause; for, if so, the excoriation from venereal gonorrhœa must be chancre, and the excoriation of a chancre must be chancre also, or, if the discharge of the latter comes in contact with the mucous lining of the urethra, it should produce gonorrhœa, whereas, either of these effects is, as far as I have seen, extremely rare. While, therefore, it is possible Mr. Hunter might overrate the comparative number of cases, which, in the absence of mercury, would be followed by lues venerea, I hold it to be extremely probable that, at the time he wrote (now about fifty years since), the venereal disease or poison possessed a higher degree of acrimony.

At page 274 of Mr. Abernethy's Lectures on Surgery, speaking of the venereal disease as described by Hunter, he observes, "This disease has almost become extinct, or is so much modified as to be unlike that which Mr. Hunter has described, and which I had an opportunity of observing in the earlier part of my life."

It is an opinion very generally entertained that venereal bubo is a sign of the absorption of the poison, or, in other words, of the contamination of the system; there is, however, I believe, little reliance to be placed on this symptom, for, in the first place, *any irritation* in the urethra or on the penis may produce a bubo; in the next, secondary symptoms frequently follow a sore without bubo: in the third place, bubo, or inflammation of the glands in the groin, or indeed of any part of the body, frequently arises from cold or rheumatism, or any like common causes. While such explanations are at hand, why have recourse to hypothesis, for after all that has been said and done, I contend, that the opinion that the venereal poison is absorbed into the body, *is not substantiated by facts or warranted by symptoms.*

I shall here take leave, I trust not uncourteously, to express, freely and unequivocally, my dissent from the views and treatment of bubo as inculcated at page 205 of Mr. Judd's recent work on *Urethritis and Syphilis.* Unless it can be proved that secondary symptoms have arisen from bubo, unaccompanied by any primary sore, I am quite at a loss to comprehend the propriety of treating it as a venereal disease. Unless it can be proved in the language of Hunter, "that bubo is a venereal abscess," exactly similar in its nature and effects to a chancre, the only difference being in size; unless all this is established *by facts* (with the opposing evidence), it is fair to treat the opinion as hypothetical.

For 1st. I have inoculated a sound person three separate times from three distinct buboes (following venereal sores), without in either case producing contamination.

2ndly. "With respect to absorption, even when the disease is followed by secondary symptoms." Mr. Hunter says, "we may observe, that even the blood of a pocky person has no power of contaminating, and is not capable of giving the disease to another, even by inoculation; for if it were capable of irritating a sound sore to a venereal inflammation, no person that has this matter circulating, or has the lues venerea, could escape having a venereal sore whenever he is bled, or receives a scratch with a pin, the part so wounded turning into a chancre. For if the venereal matter be on the point of the lancet or on the point of the pin, the punctures must become chancres."

Again, as to the venereal bubo containing chancrous matter, it might as well be argued, that when suppuration takes place in any part of the glandular system during variola, scarlatina, and rubeola, such matter would possess the specific property of each disease; or, to go on with the argument, that the matter formed from suppuration of a gland, from a dissecting wound, must necessarily contain matter of the same quality as the original. The same reasoning, for aught I see, might be applied to natural poisons, the poison of the viper, &c. The treatment of bubo then, in my humble opinion (in the language of Dr. Titley), "is not to be influenced by the consideration, *founded on sheer hypothesis*, that the glandular enlargement depends upon the absorption of a specific virus."—Page 119.

2ndly. Of the state of *disposition*.

Mercury cures the action but not the disposition. "Hence when a part is contaminated, and under the disposition to disease which should show itself at a certain time, mercury will protract that period, and the disease will not show itself so long as the constitution is under the influence of mercury. But all this time the disease will not be cured. After the mercurial irritation has ceased, the venereal disposition, which has existed ever since the parts were contaminated, will come into action; that is, the disease will appear, and in this state will be cured. These principles, being established, explain at once the difficulty that

existed, whilst attempts were made to eradicate the virus." Page 433.

3rdly. Of the state of *action*.

"Mercury cures the venereal action." Until we define more precisely what is venereal action, it is impossible to speak definitely of the action of mercury, since daily experience convinces me, that many effects imputed to the venereal poison, are neither more nor less than productions of the mercury. I conclude then,

1st. That the occurrence of venereal sores while the body is under the influence of mercury, proves that mercury does not prevent contamination.

2ndly. That mercury does not prevent the disease from running a certain course. By this I mean the occurrence of lues venerea. If this be true, it follows,

3rdly. That the continuance of the mercury for three weeks after the primary sores have healed, with a view to eradicate the virus, is perfectly unjustifiable both in theory and practice.

4thly. That there is no legitimate ground for believing that the venereal poison is ever absorbed into the body.

5thly. That the majority of venereal sores are not followed by lues venerea.

6thly. That the venereal poison varies in acrimony.

7thly. That the majority of venereal sores, which are not followed by lues venerea, are, in all probability, the production of the same poison that produces true chancre, only in a minor degree of acrimony.

It was my intention to have stated the facts on which I found the opinion, that the disease called "venereal rheumatism" is, more properly speaking, *mercurial*,—a consequence of the remedy and not of the disease. As, however, these remarks have extended to a length beyond my original intention, I cannot presume to trespass farther at present on your valuable space.

FRANCIS EAGLE.

29, Poultry, Feb. 9, 1836.

NEW RESEARCHES ON THE

MOVEMENTS OF THE HEART.

THE theory of the movements and bruits of the heart still remains in great obscurity, notwithstanding the labours of Messrs. BOUILLAUD, PIGEAUX, MARC D'ESPINE, ROUANT, and MAGENDIE, each of whom has formed a different theory. The researches of the Dublin Commission, composed of Drs. CORRIGAN, LAW, NOLAN, &c., ought to give birth, if not to the best, at least to the newest explanation, which is always something in the value of an hypothesis. In the mean time, Mons. BEAU, one of the most intelligent internes of the school of Paris, after having made several experiments on cold and warm blooded animals, has arrived at conclusions, that seem to be supported by the facts which he produces, however contradictory some of them may be to our received notions. We cannot detail the experiments which form the basis of this long memoir, but will refer to them in such a manner as will render our description of M. BEAU's ideas intelligible.

The heart is the seat of two principal movements, which alternate with each other, and on whose existence all writers are agreed; in one, the inferior part of the heart or its apex is carried forwards; in the other it is the superior part or base of the heart which advances, while the point retires towards the dorsal parietes of the chest; we may call the sound which attends the first of these movements the inferior sound (the first or dull bruit of authors); the second gives the superior bruit (the second, or clear sound). It is also very generally admitted that the first is a movement of contraction (systole); the second, a movement of dilatation. Laying aside the manner in which other authors have endeavoured to explain the mechanism of these two movements, and particularly of that by which the point of the heart is carried forwards against the parietes of the chest, let us see how M. BEAU renders an account of the latter phenomenon. In physiology, all reasoning should be founded on observation and experiment. This grand truth, first established by our countryman Bacon, for the natural sciences, is at the present day applied with success to the investigation of the laws that govern medicine and physiology, by two distinguished physicians, Messrs. Louis and Magendie: the necessity of abandoning hypothesis for observation is daily gaining more ground, and the time is doubtless not far off when the science of life will progress in proportion as we abandon idle speculation for the study of the phenomena of living bodies.

The first experiments made by M. BEAU were performed on frogs. On exposing the heart of one of those animals we are struck with a double movement of dilatation, which is very remarkable, and affects at

separate times the upper and lower moieties of the organ: if we examine attentively these two movements, we find they depend on the auricle and ventricle, which act in the following manner, viz.: contraction of the auricle, with dilatation of the ventricle; contraction of the latter followed by dilatation of the auricle, and the repetition of the same movement. When the heart is examined in the same animal, after the vascular system has been emptied by divisions of the aorta, we can still follow the movements of the organ for a few minutes: the auricle first contracts, then the ventricle, but not immediately: thus we observe the following series;—contraction of the auricle; repose; contraction of the ventricle; repose, more long than the former one: then continuation of the same series. Here there is an essential difference between the movements of the heart when full and empty: in the former we have a dilatation of the auricles and ventricles; in the latter case the dilatation is marked by a repose, and hence we are led to the conclusion that the dilatations of the heart are passive, and depend on the force with which the blood is driven into them. Having thus exposed the nature of the heart's movements in a state of plenitude and vacuity, the author asks "How do we explain the projection forwards of the apex during contraction of the ventricles? But here another question presents itself—Is the apex of the heart really thrown forwards against the parietes of the chest during the systole? M. Beau thinks decidedly not. Inspection of the heart in a living animal shows that at the instant of ventricular contraction the parietes of the ventricles are shortened in all directions; the apex is drawn up towards the base, and performs a movement from below upwards, and from before backwards: on the contrary, the impulse of the heart's point takes place immediately before the systole, and depends upon the manner in which the blood is driven from the auricle against the parietes of the ventricle. This is a point in which the author is completely in contradiction with the opinions commonly received; it remains for future experiments to confirm or overthrow his ideas. Let us repeat them again to avoid all misunderstanding:—

1st. In the systole we have retraction of the ventricle, without any projection forwards of the heart's point.

2nd. In the diastole there is a general expansion of the ventricle, particularly of its point, which is carried forward and strikes against the chest.

But the above propositions are not the only ones in which M. Beau finds himself opposed to other writers. If we turn to the succession of the heart's movements, we find a difference which is worthy of attention.

M. Beau, as well as all other writers, admit two movements, an inferior and a superior one; now if we analyze these two movements, we shall find each composed of two other movements, which we may represent in the following order:—

Inferior movement ..... { Dilatation of the ventricle; Contraction of the ventricle.

Superior movement ..... { Dilatation of auricle; Rest; Contraction of auricle.

Now if we analyze the succession of movements adopted by authors, we have,

Systole of auricle ..... { Diastole of auricle;
Systole of ventricle ..... { Diastole of ventricle.
Repose.

Here also the difference between the two systems is striking. M. Beau places the ventricular diastole between the systoles of the auricle and that of the ventricle; while in M. Hope's explanation we have the ventricular systole placed between the systole of the auricle and the dilatation of the ventricle, a succession which is not very conceivable.

But it may be objected that the experiments of M. Beau were made on animals with a single heart. To obviate this the author continued his researches on dogs, rabbits, and birds (experiments for which we must refer to his memoir), and found that in animals with a double heart, the movements succeed in the following order:—

Inferior movement, comprehending
- Augmentation of all the diameters;
- Projection of the point forwards;
- Diminution of the ventricular diameters;
- Return of the point to its natural state.

Superior movement
- A beginning, coinciding with the dilatation of the auricles;
- A middle repose, very sensible;
- A termination, coinciding with the contraction of the auricles.

From the above theory it is easy to deduce the author's explanation of the sounds of the heart: it differs little from that given by M. MAGENDIE: the foundation is the shock of the heart against the thoracic parietes; the inferior movement produces the first sound: the superior movement the second: so far the author agrees with M. MAGENDIE; he only differs from him in the explanation of the manner in which the two shocks are produced. M. MAGENDIE attributes one to the contraction of the ventricles, the other to their dilatation: while M. BEAU explains the former by the dilatation of the ventricles, and the latter by the dilatation of the auricles.

The first bruit corresponds to an elevation of the soft parts over the heart's point. The second bruit is not commonly attended with a similar phenomenon; however, M. BEAU assures us that in six cases he has observed an alternate elevation between the 2nd and 3rd intercostal spaces, corresponding to the second sound of the heart.

The duration of the different sounds, and the repose, as determined by auscultation, is, according to LAENNEC, as follows; in a complete pulsation one-fourth is occupied by repose of all the parts; one-half by contraction of the ventricles; one quarter by contraction of the auricles (1st and 2nd bruit). The author's researches furnish a different result. According to M. BEAU, if a pulsation measures one second, it composes a true measure *à trois temps*, and the first bruit, the second bruit, and the repose, will be represented by counting 1, 2, 3.

DERANGEMENT OF INTELLECT
RESULTING FROM
SABRE CUTS ON THE HEAD.

We extract the following curious case, related by M. BIESKE, from the last Number of the *Berlin Gazette of Medicine* which has reached us. (No. 52, Dec. 30, 1835.) Physicians in all countries are agreed on the difficulty of diagnosticating diseases of the nervous system, but surely never was a case so grossly mistaken by men of high rank in the profession, as the one we are about to detail:—

In the month of April, 1814, the author (M. BIESKE) was called on to attend Colonel Count BLUCHER, son of the celebrated Prince of that name. The patient laboured under a tendency of blood to the head, and was troubled with hemorrhoids. The symptoms were easily removed by blood-letting and purgatives, and the patient returned to the amusements of Paris, where he was then stationed; in a short time however he again began to complain of tightness and pain about the head, with impossibility of sleeping. The pulse was now quick and full; the skin warm and dry; the inflammatory symptoms were removed by mild diaphoretics and a warm regimen; however, a little confusion in the ideas seemed to remain after the recovery of the patient from this slight attack. Dr. BIESKE considered this symptom, which was soon joined by a return of anxiety, insomnia, and tightness of the head, as depending upon *hypochondria*, and hoped, both by acting upon the abdominal cavity and by sulphureous baths, mineral waters, and relief of the patient's mind, to obtain a complete cure. He took this opportunity of examining the state of the wounds which Colonel BLUCHER had received some time before at the battle of Dresden. They were healed, but the cicatrices still betrayed the extent and nature of each injury. One sabre-wound existed near the junction of the occipital and right parietal bones: it was excessively deep, and had probably penetrated through the inner table of the skull. A second sabre-cut, on the left parietal bone, seemed more superficial; a third, near the junction of the parietal with the frontal bone, on the right side, was also deep, and had probably penetrated into the cavity of the skull. There was also a cicatrix on the neck from a lance-wound, and a second on the right side of the chest: this latter had penetrated deeply into the substance of the lungs, as was shown by the violent hemoptysis and other symptoms which accompanied and followed the wound. The patient, thus severely wounded, and taken prisoner, was brought into Dresden, where he was attended and cured by Baron LARREY, Dr. OHLE, &c.

Colonel Bluchse was now exchanged for a French General; his health seemed fully established, and he followed the allied army amidst all the fatigues of a winter campaign from the Rhine to the walls of Paris.

In the month of May, 1814, the patient left Paris, with the intention of taking mineral baths, but was prevented by some indisposition; he also now conceived the strange idea that he was possessed of a secret which would preserve the kingdom of Prussia from all dangers, and that his neighbours were constantly at work either to force this secret from him, or to deprive him of life, and discover it in his entrails. It was impossible to convince the patient, by any moral reasoning, of the folly of the ideas by which he was possessed; his physician, therefore, ordered some compound tinct. of bark, with tinct. of rhubarb and wine. The patient now, for a short time, thought himself cured, but his irregular ideas soon returned, and it became necessary to hold a consultation of the most eminent physicians in Berlin; their prescriptions, however, were not followed by any beneficial result, and the patient continued to regard them as fiends and persecuting demons. Dr. Bieske, who still preserved some share of the patient's confidence, hoped to obtain an amelioration, by acting on the intestinal canal, and by exercise; yet it was impossible to overcome the morbid mistrust which constantly occupied his mind; in the intervals of lucidity, he often complained of confusion in the head, vertigo, and sparks crossing the eyes.

Here we cannot help remarking on the stinacy of his medical attendants, in attributing all these symptoms to hypochondria, instead of turning their attention to the state of the brain, which had evidently suffered from the wounds received at the battle of Dresden. Far from pursuing an antiphlogistic treatment, the opposite method was pursued with a fatal perseverance; thus (to resume our author's history of the case) Dr. Bohn, one of the attending physicians, advised him to drink champaigne; under the stimulating influence of which beverage, he felt himself comfortable for a short time; but, on the following night, the patient was very uneasy and agitated, and the attacks of vertigo were excessively violent; the pulse was now full and hard; the face and eyes were much injected; the circulating system was greatly excited. Dr Bieske ordered some blood to be drawn from the arm, but the patient obstinately refused; he was, therefore, obliged to apply twelve leeches, and recommend a more cooling diet. The patient now felt much better, his appetite returned, and, faithful to the ordonnance of Dr. Bohn, he drank *every day* a bottle of champaigne!!!

The baneful effects of such treatment were not long without manifesting themselves; the vertigo again appeared with excessive violence, and M. Bohn was compelled to order venesection, with a cooling diet, and the use of sulphur. After the abstraction of some blood, and the administration of a few doses of sulphur, the patient found himself much relieved; however, the sudden change from champaigne to cold water inspired his mind with fresh distrust; he now refused every kind of medicament, determined on treating himself, and for this purpose made a journey on foot into Silicia, from which he returned in eleven days much more ill than he set out.

The patient's state now seemed very serious, and a fresh consultation of the Berlin physicians was called by Prince Blucher. On the 14th of November, 1814, the patient was examined with care, and the physician-general gave it as his opinion, that the disease had no connection with the wounds received at the battle of Dresden, but consisted in an aberration of the intelligence, which would best be treated by occupation, and the effect of moral agents. These latter, however, had no influence on the disease; the patient became so unruly and distrustful, as to endanger his own life and that of others; he often walked about at night with loaded arms, and once challenged a superior officer in duel, who, he imagined, threw some shade on the glory of his father. A medical examination of the patient again took place, in consequence of this circumstance; a decided opinion of mental derangement was given, and the duel of course prevented; this enraged him to such a degree, that he formed the resolution of insulting, or even killing, his adversary, whenever he might meet him. This design was frustrated by proper precautions, and the impossibility of avenging his honour had such an effect on the unfortunate patient's mind, that in the midst of a breakfast given to his friends, he fired a pistol loaded with ball into the left side of the chest near the clavicle; the wound, although dangerous, was not mortal; the bullet did not penetrate through and through the chest, but seemed to remained lodged under the left scapula; the hemoptysis and other symptoms evidently showed a wound of the lungs, of which however the patient was completely cured. It is unnecessary to go through the various methods of treatment by turns proposed, but all fruitlessly; confined at one time, at another allowed absolute liberty, the patient was at length sent under proper care on a journey into the neighbourhood of the Rhine, but returned after a lapse of a year without any benefit.

He now lived without observing any regimen; appetite good; slept well, and seemed to enjoy excellent health. However, he seemed frequently to suffer in the head; the hand was constantly carried to the forehead or head, and the patient at these times expressed his suffering by crying, "Oh, God, my head" (O Gott, mein kopf). His folly now became of an absurd kind; he believed, not only that his food and drink were poisoned, but that a portion of his strength was removed each time the hair was cut, and hence refused to be shaved or have his hair cut; he also conceived that a part of his secret was contained in his excrement, and always satisfied nature in a remote and unfrequented place, after which he carefully buried the precious deposit.

In this melancholy state the patient survived, without any remarkable change, from the year 1814 to the 10th October 1829. The author does not make any allusion to the symptoms which immediately preceded death. The body was examined on the 11th of October.

Autopsy.

External Examination.—A deep cicatrix, two inches long, on the right parietal bone; a second of similar extent near the junction of the parietal and occipital bones on the right side; a third cicatrix one inch and a half over the left parietal bone; on the body a large cicatrix adherent to the second and third ribs on the left side of the breast near the clavicles; behind, near the twelfth dorsal vertebra, two cicatrices; above the right elbow-joint, a transverse large cicatrix adherent to the os humeri; the fingers of the right hand, except the thumb and index-finger, were contracted, and united together by cicatrices; on the outer side of the left calf a round cicatrix, which seemed to have resulted from a gunshot wound, a second over the left knee-joint, from a penetrating wound. On removing the skull-cap the dura mater was found to be united to the bone more closely at the points corresponding to the wounds; the anterior one had evidently penetrated into the cavity; the posterior cut on the right parietal bone had also penetrated, but not that on the left side. The whole anterior surface of the hemispheres, superiorly, was covered with a whitish firm layer, about the thickness of a knife-blade, and similar to that produced by inflammation. The base of the brain presented nothing abnormal. On dividing the substance of the brain the white matter was found somewhat injected, and the ventricles contained a little more fluid than usual. The cerebellum healthy.

In the cavity of the chest the lungs were closely united to the costal parietes; in the midst of the substance of the left lung, opposite the third and fourth ribs, was found a leaden bullet, which appeared in several places as if it had been cut with some sharp instrument. The third and fourth ribs had been evidently fractured inwards, and were united in an obtuse angle by bony deposit. The examination of the other organs revealed nothing worthy of notice.

The cause of the lesion of intelligence in this curious case, was evidently a chronic inflammation of the membranes covering the cerebral hemispheres. Unfortunately this cause was completely overlooked by the physicians who attended General Blucher, and a treatment was adopted which was rather calculated to aggravate than to allay the morbid irritation of the sensorium. This is the more remarkable, as the French surgeons who treated the patient in 1813, after the battle of Dresden, expressed their fears that at some future time the wounds of the head might give rise to a derangement of the intellect. The length of time (sixteen years) during which a large musket-ball remained imbedded in the substance of the lung without giving rise to any symptoms, is also another curious circumstance.

To the Editor of THE LANCET.

SMALL-POX AND VACCINATION HOSPITAL, ST. PANCRAS.

Report of Dr. GREGORY, *Physician, issued Feb.* 1836. (*Abridged.*)

DURING the past year 401 persons have been treated within these walls, and so many centres of infection removed from the metropolis. Of that number 89 have died, and 312 have been restored.

4140 persons were vaccinated at the hospital in 1835; and 1767 persons, chiefly medical practitioners, were supplied with vaccine lymph, for use, at home and abroad.

The records of the hospital, during the past year, sufficiently attest that the hopes once entertained of banishing the small-pox from the earth are visionary. Thirty-six years have elapsed since the general diffusion of vaccination throughout this country; yet small-pox still exists, and, by the bills of mortality, may be shown to have proved fatal to 863 persons in London alone during the last year. In various parts of the country it has shown itself during the same period. Nor do other quarters of the globe enjoy an exemption from the disease. On the continents both of Europe and Asia small-pox has recently occasioned considerable devastations.

The experience of the past year shows,

certainly, that the confidence of the public in and about London in the protective power of vaccination has in no degree diminished. The numbers vaccinated at the hospital in 1835 exceed that of any former year, and in no instance has any doubt or distrust been manifested by the parents of the children.

The records of the hospital show further that the security of vaccination, though so remarkable in the early periods of life, is not so complete and permanent in after-life as at one time might have been anticipated. 144 persons labouring under small-pox have been admitted during the year, who in early life have been vaccinated. All of them, with few exceptions, were adults, an interval, varying from ten to thirty years, having elapsed since the date of their vaccination. Two-thirds of this number had small-pox in a mild form, wholly devoid of danger. The remaining third were less fortunate, having passed through the disease with greater or less degrees of severity. Still. the mortality in this class of patients was very small, hardly amounting to 5 in each 100; while the unvaccinated perished in the proportion of 33 in 100. These considerations tend to show that vaccination is still entitled to the highest praise as a means of diminishing both th quantity and the severity of small-pox: that it is an object of national importance to encourage and foster it; and, lastly, that the attention of medical men cannot be too strongly called to the necessity of carefully selecting the lymph they employ, and accurately investigating the several causes on which its diminished efficiency in the more advanced periods of life may depend.

31, Weymouth-street, Feb. 4, 1836.

THE LANCET.

London, Saturday, February 20, 1836.

ANOTHER inquest has been held upon the body of another victim of the satanic system of *quackery*, and another upright English jury has returned a verdict of "manslaughter" against one of the parties implicated in the abominable proceeding.

But how is it, we ask, that the chief culprit is allowed to escape in these atrocious malpractices? How is it that the principal offender is allowed to pass unscathed from the inquiry, and that the mere agent, the ignorant tool of the mercenary destroyer, is the only party punished?

Many complaints have reached us respecting the conduct of the Coroners at these quack victim inquests. Charges of ignorance, allegations of partiality, and imputations even of corruption, are thrown out against some of those functionaries. That non-medical Coroners must act as blockheads in their offices, is a necessary consequence of their ignorance; but we had hoped that they would so conduct themselves as at least to remain free from accusations of knavery. That many of the non-medical Coroners are most respectable men we freely admit, and hitherto we have seen no just ground for excluding Mr. BAKER from amongst that class. We must freely tell him, however, that his behaviour at the late inquest on the body of Captain MACKENZIE, at the Ratcliffe workhouse, has been viewed by many persons with suspicion, and that reports unfavourable to his official character on that occasion, are in a rapid course of circulation. If Mr. BAKER be capable of forming an opinion on such a subject, we would ask him what he would think of the moral principles of a man who, in a sound state of mind, would advertise *one* medicine as a cure for *all* diseases. If Mr. BAKER be not lost to reason,—if his mind be not divested of every portion of common sense, he must know that if there be people so devoid of understanding, so credulous as to place any belief in the infamous puff of the mercenary quack who sets forth such an advertisement, they must, necessarily, in numerous instances, pay the penalty of DEATH for their folly.

Under this view of the case, the crime ought to be brought home to the offender in the first degree, and whenever it is *proved* in such cases that the cause of death is to be found in the poison, or in the effects, of Morison's Pills, THE VERDICT OF MANSLAUGHTER SHOULD BE RETURNED AGAINST MORISON HIMSELF. This would be an application of the law to the source of *one*

of the calamities which is at this moment so severely affecting society. Is there any thing so redeeming in the character and conduct of MORISON, that he should be made an exception to the ordinary rules of law at coroners' inquests? Ought he not to be treated as a Fellow of the College of Physicians, or a member of the College of Surgeons, would be treated under similar circumstances? Take the case of either of those legally qualified practitioners, and let us see what would be done. Say that one of the parties in question imagines that he has discovered a remedy for all diseases. He prescribes it in a multitude of cases. It is proved by indubitable testimony, that death, on several occasions, has been the dreadful result of the use of the medicine. Still the practitioner persists. He puffs his own skill, extols the power of his nostrum, and directs the medicine to be again taken. A chemist supplies the drug, in conformity with the direction of his employer. The unsuspecting patient swallows the poison and dies. An inquest is held on the body. Who was guilty of the manslaughter in this case? The chemist or the doctor? The principal or the agent? There can be but one reply to such a question.

But is it *manslaughter?* Is it not *murder?* We repeat the deliberate and solemn inquiry,—Is not the person who kills under the circumstances we have just stated, guilty of committing the crime of murder? We believe, that some of the best-informed jurists in this kingdom will answer in the affirmative, as we are not aware that any line of distinction can be drawn between the man who, in order to make money, destroys life by poison, and the man who, with a view to enrich himself, plunges a knife into the bosom of his neighbour.

We shall not at present pursue this subject further, with reference to any inquests which have been recently held, but we hope to be able to publish in the next number of THE LANCET, a report of the evidence which was taken at the late inquest on the body of Captain MACKENZIE, which concluded, after several days' sittings, on Wednesday last, some hours after midnight, in a verdict of "manslaughter." But we ask, Is society to continue to endure this evil without making an effort to throw it off,—a calamity almost as fatal in its results as the plague or the cholera? Whatever is secret in medicine is based in knavery, and we unhesitatingly state that the time has come when the system of quackery in medicine must be put down by the arm of the law. At any rate an attempt shall be made to ascertain whether the law be powerful enough to arrest the proceedings and subvert the machinations of a few mercenary and sanguinary adventurers, who now, to the disgrace of the medical profession of England, circulate their baneful and odious trash under the sanction of a Government stamp. Cannot the members of the profession petition both Houses of Parliament for the suppression of quackery? Ay, can they! and with every prospect of success. In order to accomplish this object, however, there must be co-operation among medical men. Unity of action and energy must be manifested in giving the proceedings proper publicity and influence. If they consider that quack medicines ought to be sold, let them remain passive. If, on the other hand, they consider that the use of quack medicines is injurious to the welfare of society, and is the cause of death to hundreds of their fellow creatures, they are called upon by every sense of duty, justice, and humanity, to take immediate measures for the suppression of this no longer supportable evil.

THE Charter of the Metropolitan University will be laid on the table of the House of Commons on or before the 24th instant. A letter, written by Mr. WARBURTON, on the subject of the contents of this instrument, to the LORD PROVOST of Edinburgh, was referred to in the *Globe* newspaper

about a week since. In the absence of accurate information on the subject, Mr. WARBURTON has merely adverted to what may probably constitute some of the conditions of the Charter, in general terms. Ministers may be assured, that no member of the legislature could have been consulted with greater advantage than Mr. WARBURTON. At any rate, that gentleman will have the consolation of knowing, that if the new Charter should fall short of the expectation of the literary portion of the public, he will in no respect be concerned as the author of the disappointment. It is deeply to be regretted, that Ministers should feel that they are too tightly bound down by old customs to make the Parliament acquainted with the principles on which they are about to exercise the royal prerogative in founding a Metropolitan University. It is ever the case in this country, that although we see the coming evil, we have not the power to avert it, though, to be sure, after it has arrived, we have the privilege of complaining, which must be a great consolation to the people of a free nation.

The following is the statement which appeared in *The Globe*:—

"EDINBURGH TOWN COUNCIL, TUESDAY.—A letter from Mr. Warburton, M.P., in answer to a letter from the Lord Provost, was read, in which he assures the Council that no one could be more anxious to prevent the establishment of any new monopoly of teaching, and to see instituted some uniform system of medical instruction and examination, to serve as a basis for granting to persons properly instructed and examined the enjoyment of equal professional immunities in every part of the United Kingdom. Whether such a plan could be carried into effect by means of any Crown charter, might reasonably be doubted. He thought government would scarcely be justified in delaying to constitute the proposed new University of London, for the purpose of maturing beforehand, and incorporating with the scheme of that university, a comprehensive measure of medical reform. The Government, he thought, should constitute the new University, investing it with all the privileges appertaining to such institutions, reserving to itself the power of modifying its laws and ordinances, so as to render them generally consonant to any general plan of medical reform that may hereafter be approved of. Except upon certain points, he was not informed of the details of the proposed plan. The most important that he was aware of, as affecting the interests of medical schools in general, was, that no medical school whatever was mentioned in the charter by name. A matter of importance for which the London University was contending was, that no teacher of any medical school, and no medical or surgical officer of any hospital, shall be a member of the Board of Examiners. The Council directed copies of this letter to be communicated to the Secretary of the University for the information of the Senatus, to the Royal College of Physicians, and the College of Surgeons."

A BODY of young gentlemen having admission to the halls of the *University of London*, have, after the lapse of a month, come forward in aid of the plea that public examinations should form an imperative feature in the constitution of our governing medical bodies. These gentlemen formed no part of the meeting of medical students who were assembled at the Crown-and-Anchor Tavern on the 18th instant, for on that occasion the resolution for a petition to the House of Commons in favour of public medical examinations was carried unanimously, and the Chairman was directed to sign the document on behalf of the meeting, without ten hands being raised against the proposition. There is this difference, however, between the proceedings of the thousand and some hundred students who were assembled in the Strand, and the party of students who have since done something else in support of the demand for public examinations. The prayer of the one must be regarded by all as the emanation of a noble courage; the protest of the other is the result of what too many will ascribe to youthful fear. The demand of the one is addressed to the House of Commons; the declaration of the other to the House of Apothecaries. The cry of "marked men" has been raised at the University of London, and the timid and the apprehensive have fallen at the feet of the Examiners at Blackfriars-bridge, with

this assurance:—"We aided not in the condemnation of your secret proceedings in "general, or of your private rejection of "our fellow-student Mr. SMITH, in particular."

In speaking of the document just submitted to the view of the Apothecaries' Company as an aid of the prayer,—and a most valuable aid it is,—of the great meeting of students, we do so from regarding it as an important proof of the iniquitous power and influence possessed by those secret tribunals which can, by exciting the terror of *some* candidates for examination, extort such a declaration from one third in number of the intelligent students of the most liberal public seminary for instruction in the kingdom.

WHEN a black man encounters a lion, or meets with a bull, he lies down on his back, and pretends to be dead, believing that the giant animal will then only smell and pass on. In the College of Physicians,—to go from a great topic to a little one,—Sir HENRY HALFORD enacts the trembling man, and medical reform the lion, and whenever the lion wags his tail, the cunning President pretends to sleep. But the lion will eat him up notwithstanding. The last occasion on which Sir HENRY "slept" in the chair, was during the late motion "that "Licentiates of five years' standing should "be eligible to be balloted for as Fellows." The mover and seconder having enforced the proposition as a measure of reform which was particularly appropriate at a moment when it was desirable to convince the Government that the disposition of the College was decidedly *liberal*, arguments in support or reply were awaited, or a tacit assent at once to the motion. However, it had been arranged beforehand by Sir HENRY, that nobody should reply, that the President should pretend to sleep, that every refractory fellow should be borne down with a cry of "question," and that even this trumpery motion for reform should be promptly rejected by the ballot. True to the word of command, the resolution was negatived by a majority of nearly three to one. The result was communicated to the CHANCELLOR of the EXCHEQUER.

The majority was partly composed of the following members of the Halford faction, amongst others:—Drs. LATHAM, WATERFIELD, WATSON, HUE, FANNY, MACMICHAEL, TURNER, and GEORGE MANN BURROWES, whose name deserves to be written in full,—while Drs. CHAMBERS, SEYMOUR, M'GRIGOR, HEWITT, and even Dr. PARIS, having begun to see the signs of the times, purposely staid away. Drs. LATHAM and WATSON were particularly conspicuous in their outcries against any discussion of so contemptible a subject as reform in the College.

UP to the present hour, we have received no denial from Dr. W. CUMMIN, of the "Aldersgate School," of the correctness of the allegation, that he is the official colleague of Dr. MACLEOD in editing a journal which has been systematically devoted, from the first moment of its existence, to slandering the general practitioners in medicine in this empire, and, latterly, especially, to calumniating and vilifying, and fabricating falsehoods directed against, the characters of the students of medicine assembled in the metropolis.

LATE ELECTION

AT THE

RICHMOND HOSPITAL, DUBLIN.

WE had not room last week for any part of the letter of our correspondent *Observator* in reply to the letters of Messrs. CRAMPTON, COLLES, and CARMICHAEL. We now, however, insert an analysis of its contents:—

Observator says that he felt it to be a matter of duty to publish the account which appeared under his signature,—not designing to inflict obloquy on the gentlemen concerned, but to describe facts with fidelity, desiring at the same time to afford the implicated parties an opportunity of publicly rebutting, if possible, the statements which prevailed against them in Dublin. He says that he believed and still believes that his description of the former proceedings as regards hospital elections, of two of the Commissioners, Messrs. CRAMPTON and COLLES, were such as should have prevented their nomination on

this occasion, and he alleges that those two Commissioners in their reply have not relieved themselves from the odium of the allegation that they are "jobbers in public medical offices." For instance, he says that at the *Meath Hospital*, Mr. CRAMPTON and three of his apprentices (one a nephew) fill four of the six medical offices. *Observator* argues that the *appointment* of the "Commissioners," though denied, is proved by their admission that they carried on an "official" correspondence with the government on the subject of Mr. ADAMS's fitness for the vacant office. He adds, "I beg also most distinctly to state that Mr. ADAMS was not a candidate until *after* the Commissioners were appointed," Mr. ADAMS himself declaring to his friends that it would be useless to annoy himself by any application on the subject, as he would have no chance of success with the present Government; and *Observator* says that he has reason to believe that Mr. ADAMS became a candidate solely in consequence of receiving a communication from one of the Commissioners immediately after their appointment. It must, however, be perfectly plain to *Observator*, that some of the statements in his letter form no reply to the third paragraph of the letter at page 680 of THE LANCET.

Observator stated in his former letter that the commissioners wrote to the government, recommending Mr. Adams's appointment on the grounds that it was necessary for the support of "the Medical School of Dublin." He says, "I now reassert that they did do. This official letter was in Mr. ADAMS's own possession for some days, and he took great pains to exhibit it, as a trophy, to his friends, more especially to his pupils, as evidence in favour of his capability as a lecturer."

With regard to Mr. CARMICHAEL, *Observator* thinks that the following passage in Mr. C.'s letter justifies a considerable portion of the remarks made on the resignation of Mr. CARMICHAEL: I resigned my situation of attending, for that of consulting surgeon; because, by doing so *I secured* the appointment of two gentlemen, whose long connection with the hospital, and well-tried competency and worth, had been fully established; while, by the arrangements that have been subsequently made, I shall continue my visits to the hospital, &c."

With respect to the remark of Mr. CARMICHAEL on the long connection of Messrs. Adams and M'Donnell with the Hospital, *Observator* says, "I am well aware that those gentlemen have been attached for some time to a school in the neighbourhood of the hospital; but it is a perfectly *distinct* establishment, and not even situated in the same street with it; nor did they ever hold in it even the offices of assistant surgeon." Respecting the testimony of Mr. CARMICHAEL that he has always heard Mr. ADAMS express liberal sentiments on political subjects, Observator says that whatever Mr. ADAMS, may have expressed, he always voted in Dublin for anti-reform candidates. *Observator* was not the author of the letter in the *Freeman's Journal*, nor was he aware of its existence until he read it as a note to Mr. CARMICHAEL's letter.

Observator concludes by saying,—"You have mentioned in a late number that the authenticity of the petition presented to the Government, purporting to come from the Sisters of Charity, in favour of Mr. FERRALL, has been denied. I believed the petition was *genuine*."

We have here reduced into a comparatively brief space, those points in the letter of our correspondent, which we deem it fair, apart from the authority of his name, to publish in reply to authenticated documents, adding that while we know our correspondent to be a gentleman of the highest respectability in one of the professional circles of Dublin, there can be no doubt that he was misinformed on some points discussed in his former communication. With regard to Mr. CARMICHAEL, we feel bound to say that we have never before heard even a charge of jobbing in medical elections brought against him, and we are perfectly sure that no surgeon in these kingdoms will hail with greater pleasure the adjustment of medical law and hospital appointments on a just and honourable basis, with greater delight than that gentleman. At the same time we express a belief, founded on pretty good grounds, that no very long period will elapse before the task of exposing errors of medical migovernment under the present system, will no longer need to be one of the duties of the medical reformers of Dublin.

PRESENT TO SIR CHARLES CLARKE.

To the Editor.—Sir, You have been unceasing in your endeavours to elevate the medical profession in public estimation, by fearlessly exposing, on the one hand, its corruption and malpractices to ridicule and contempt, and, on the other, materially promoting its interests, by showing a praiseworthy liberality in commending good conduct, and I, therefore, am convinced that you will derive a satisfaction in communicating to your numerous readers the following event, so honourable to the eminent individual who forms the chief object in it, which has lately taken place in this county,

Sir Charles Clarke, ever since he became one of our land-holders here, has resided amongst us during the greater portion of each year, and it is not surprising that his high professional character should have prompted not only the medical practitioners in his neighbourhood to seek his advice in all cases of difficulty and danger, but that his council should be equally desired by our nobility and gentry. His active disposition made him always ready to attend to the calls of distress, and, to his credit be it told, he has been in the constant habit of lending his valuable services to all those who have sought his advice, without even excepting a fee. In consideration of this liberal conduct, the great bulk of the *ladies* of Norfolk unanimously agreed to present Sir Charles with a piece of plate, for which a large subscription was immediately made, and an order given to Storr and Mortimer, the goldsmiths, in Bond-street, to execute the same from a design made purposely by Chantrey, the eminent sculptor. I have the honour to be, Sir, your obedient servant,

CHIRURGUS.

—— House, Norfolk, Feb. 12, 1836.

KING'S COLLEGE, STRAND.

To the Editor.—SIR,—As your numerous readers are extremely anxious at this moment to become acquainted with every circumstance that can elucidate facts relative to the conduct of certain individuals who pretend to be *liberal*, you may perhaps think it worth mentioning that the Worshipful Company of Apothecaries are *proprietors* in King's College, Strand. This simple fact may of itself be sufficient to account for the King's College students having come forward on the late occasion to *laud* the *disinterestedness* of the Rhubarb Hall examiners.

I have also heard it rumoured, that in consequence of the want of money at the institution, Sir Henry Halford has been making an indirect application to the proprietors of the Drug Shop in Blackfriars, to get them to purchase some more King's College shares. I am, Sir, yours, &c.

AUSCULTATOR.

Piccadilly, Feb. 16th.

PROFESSIONAL ADVERTISEMENTS.

To the Editor.—SIR,—While paying a professional visit this morning to an elderly gentleman of fortune, a very small publication, showily bound and gilt, containing three "Introductory Lectures," by Mr. Evans Riadore, was placed in my hands. I felt much at a loss to elucidate the mystery, as my patient said that Mr. Evans Riadore was totally unknown to him, and that he could in no way account for the present. I thought at first that there must be a mistake, but in the title-page it was plainly enough addressed

"To —— —— Esq., F. A. S. From the Author."

I then suggested that there might be another F. A. S. of the same name as my patient, or that Mr. Evans Riadore, being an F. A. S. himself, he had sent his *brochure* (a scarcely allowable proceeding, however) to the rest of the fellows; but, on examining the list, neither of these surmises was found to be correct. Upon a cursory examination of these lectures, I observed that they were composed with a considerable regard for effect, and that the author's address was duly given. Now, Sir, I beg to inquire the meaning of all this; and whether Mr. Evans Riadore has presented his introductory lectures to any other gentlemen who are totally unknown to him; and, in fine, whether it is becoming in a member of a liberal profession to follow in the track of the circular-circulating wine-merchants and merchant-tailors of the metropolis?—I am, Sir, yours &c.,

INVESTIGATOR.

London, Feb. 16, 1836.

METROPOLITAN MEDICAL UNION.

To the Editor of THE LANCET.

SIR,—In my address on the state and wants of the medical profession, delivered in the Council Room of the Town Hall, Ipswich, in the year 1834, I most earnestly recommended that a provincial association, similar to that already existing in the western division of the kingdom, should be formed. This, with me a very favourite and important object, has been most happily accomplished; but it seems that something yet remains to be effected. The western and eastern associations require a point or bond of union; and where could this be so completely established as in the metropolis? I do most sincerely hope that some leading members of the profession in London will immediately set about the formation of a Metropolitan Association, similar in principle to the Eastern and Western associations. For general and literary purposes we might unite, while we may hold our annual meetings in our own provinces, with the exception of every third or fourth year, when we might meet our metropolitan brethren in London.

Favour me by allowing a space in the pages of THE LANCET for the insertion of this suggestion, and if it should be so fortunate as to meet with your approbation, I trust that you will support it with your

wonted ability. I have the honour to remain, Sir, yours very respectfully,

J. BEDINGFIELD.

Stowmarket, Jan. 26th, 1836.

EULOGISTS OF RHUBARB HALL.

To the Editor of THE LANCET.

SIR,—In Roderick's journal of last week is printed a document headed "*Protest* of the Students of Guy's Hospital against the late proceedings at the Crown-and-Anchor" said by Roderick to have attached to it the signatures of 94 of the students of that hospital:* now Sir, I think it right that you should be acquainted with the hole-and-corner manner in which this "protest" has been got up by its promoters, Messieurs H. C. Muggand and J. Bent. These buzzing busy bodies, who attend to every person's affairs but *their own*,—or they might perhaps, ere this, have been enabled, with the aid of a little favour at court, to pass the ordeal at Rhubarb Hall,—have got up this document expressly to court that very favour. I am sorry that their abilities and diligence need it. In that document they term the 94 who have signed it, a "large majority" of the students of Guy's, but to say the least of it, this is untrue. They also call the proceedings of that meeting "ill judged and *violent*," and state that they were "characterized throughout by party violence," which is notoriously false. Now, Sir, from the admission of two individuals whose names are attached to that document, I can assert that neither of them was present at the meeting; consequently they rely on the reports of others in making the allegations to which they have attached their names; and I believe that very many more, if not the majority of those who signed that paper, were not present at that meeting; and I can positively say that not a few repent having signed it; I have learnt this from their own lips. In addition to this, I may remark, that Messrs. Muggand and Bent induced several juvenile pupils (fresh from the country) to sign the paper in question, solely on the ground that they could not do wrong by following in the footsteps of their seniors. These young gentlemen will learn wisdom from experience. I refrain from saying any thing more on the subject of this "protest," than that we are greatly surprised in the Borough that any medical students, of whatever party, should be found to connive at and applaud the ungentlemanly and unworthy manner in which the worshipful examiner at Apothecaries' Hall acted when he questioned and rejected Mr. Smith. I am, Sir, yours very respectfully,

A GUY'S HOSPITAL STUDENT.

Guy's Hospital, Feb. 16th, 1836.

* 94 does not form a fourth part of the number of 400, which *Guy's Hospital* used to boast that it fostered per session. No wonder that medical reform is a repugnant topic in some nooks in that quarter.

NOTE FROM MR. WATTS.—*To the Editor.*—Sir,—On reading over my letter in your excellent publication, I beg to say that I committed a quite unintentional mistake, when I stated that I had attended nine parishes for eighteen years. Several of them I have so attended, and others in intermediate spaces of time. This was what I designed to express. At the same time I beg to assure you and my professional brethren that my object in giving publicity to the Wheatenhurst Poor-Law-Union proceeding, was not with the slightest intention of doing Mr. Moseley any professional injury, but to point out the system as one which is degrading to the profession, and injurious to the poor. I am, Sir, your obedient servant,

THOMAS WATTS.

Frampton-upon-Severn,
Feb. 17, 1836.

CORONER FOR ABERYSTWYTH.—*To the Editor.*—Sir,—Seeing by a letter inserted in THE LANCET of Feb. 13th, that you are anxious to ascertain the number of medical coroners in England and Wales, I beg to state that the Coroner for Aberystwyth, Cardiganshire, is Rice Williams, Esq., a medical gentleman who has filled the office of Coroner in that town for a length of time. I am, Sir, yours, obediently,

A STUDENT OF MEDICINE.

MEDICAL WITNESSES BILL.

To the Editor.—Sir, I enclose you a copy of a Petition to the House of Commons, prepared at a meeting of the committee of the "Bucks Medical Association," lately held at Aylesbury. It is now in course of signature, and will shortly be ready for presentation. The support of its prayer will be earnestly requested of the county and borough members. I have the honour to be, Sir, your obedient servant,

ROBERT CEELY, Hon. Sec.

Aylesbury, Feb. 15, 1836.

*** The petitioners draw the attention of the House to the fact that a coroner's inquest, without efficient medical evidence, can be but an empty form, and is, consequently, a source of useless expense, and

"in common with the medical profession, they consider it a great hardship that the law does not provide any remuneration for their important services." They accordingly conclude the petition by "respectfully, but earnestly, beseeching the Honourable House to afford them substantial relief from so manifest an injustice, by the enactment of such a measure, as the Honourable House, in its wisdom, shall think fit."

The promptness and spirit with which this petition has been framed, deserve universal imitation in the profession.

HOUSE OF COMMONS.

Thursday, Feb. 18th, 1836.—Mr. WAKLEY this day gave notice of a motion for the 17th of March, for leave to bring in a bill for suppressing the sale of secret medicines, commonly called "quack medicines."

NEW METROPOLITAN UNIVERSITY.

Mr. WAKLEY.—Sir, the petition which I hold in my hand is from the Rev. JAMES ALEXANDER EMERSON, of the Hanwell Schools, in the county of Middlesex. And the petitioner states, that he has heard with surprise and regret that it is intended to limit the terms of the proposed Metropolitan University Charter to two institutions in London. He therefore prays, that the House will take into consideration the injustice to other scholastic institutions, of granting such exclusive privileges to those two. Sir, I will take the liberty of saying a word or two on this subject. It is very well known that, in April last, it was resolved by this House that an Address should be presented to the Crown, praying that his Majesty would be pleased to grant a Charter to the London University. On that occasion, a very large majority of the members of this House voted in favour of presenting that address; and solicitations came from all parts of the United Kingdom for the establishment of such an University, in consequence of the manner in which the Dissenters had been excluded from the privileges of the universities of Oxford and Cambridge. Sir, I do not complain of those institutions, and I, for one, never will consent to the throwing of power into the hands of those who are hostile to the proper authorities of the Universities of Oxford and Cambridge, or adverse to those institutions. (*Hear, hear, hear.*) But I would ask, in establishing a new university in this metropolis, are we to have another scholastic monopoly set up in this kingdom? And if a Charter is to be granted, which is only to embrace two or three establishments, surely the privileges so conferred must be considered as a monopoly, because others will be excluded from the like enjoyment. I regret exceedingly that the Chancellor of the Exchequer is not here, as he promised me that he would be, to state to the House what are his intentions on the subject. The House having determined on voting the address to the Crown, which I have mentioned, is it right, I would ask, that the conditions of this new charter should be framed in secret, and that the members of the House are to know nothing of its conditions until the charter has received the sign manual? If that be the case, the abuse of the Royal prerogative cannot too soon be corrected. (*Hear, hear.*) Indeed I hope the House will now interfere in this matter, and call for a draft of the charter to be laid before it previous to its receiving the sign manual, for such a legal and authoritative instrument should not be promulgated until at least the legislature has been made aware of its express terms. (*Hear, hear, hear.*) In the constitution of the new university, there can be no law that will compel the students to reside under the immediate observation of the professors, a regulation so essential to their correct government and moral conduct; and yet, if the charter be one of exclusion, the students at the private schools, where the dormitories are under the direct control of the masters, will be excluded from the honours of the university, while those who are under no personal restraint or moral management on the part of the professors will receive all the benefits of the institution. I will only add that I trust Ministers will candidly and honourably state to the House what are the conditions of the Charter, because although it is easy enough to say that his Majesty's Ministers will hold themselves responsible should it contain any improper clauses, yet I do not know how they are to be made so, either to this House or to the public, when the document is completed. I beg to observe, in conclusion, that the gentleman from whom this petition comes, requests me to state that he forwards it without entertaining any political feeling whatever on the subject. He has none. He is sincerely attached to the Universities of Oxford and Cambridge, and he desires only that the public good may be consulted in the measures that may be adopted. (*Hear, hear.*)

Sir GEORGE ..... said, that he could assure the House that the people of Scotland were extremely urgent that the disabilities under which the Dissenters at present laboured, should be removed as soon as possible.

Mr. TOOKE said, he apprehended that the petitioner had put an erroneous construction on the intended Charter, the petitioner believing that because two schools only would

be nominated in the Charter, viz., the London University and King's College, that therefore the privileges of the Charter would be confined solely to those schools. The fact was that power would be reserved to the Examiners of the Metropolitan University to name, from time to time, such other schools as should be considered, from their standing and reputation, fit to be added to the number of privileged schools, and which should be entitled to participate in the benefits and honours to be derived from the Charter.

The petition was then ordered to lie upon the table.

THE NEW QUACKERY.—*Westminster Medical Society*, Feb. 13.—HAHNEMANISM.—We have room this week only for the following:—

Dr. JOHNSON having moved the adjournment of the debate on this subject last week, he was now requested to take it up. In doing so, he briefly said that homœopathy, he believed, was not the real subject of debate on the last evening; but the proceedings then, rather formed an episode to it, something like a romance engrafted on history. He should leave others to hunt after the bubble, believing that it would burst in grasping. The doctrine of Hahnemanism was founded in error, and too often pursued in knavery, and he was convinced that the homœopathists treated the more urgent cases with our own remedies, and after our own fashion; yet who could expose the fact, for they took the precaution to furnish their patients not only with advice, but with the medicines. When they got old chronic or nervous cases, and not unfrequently *imaginary* ones, the doctrines of Hahneman were pursued. Chiefly from these reasons, he (Dr. J.) pronounced it to be a system of knavery and deception.

THE governors of the institution entitled the "Free Hospital," situated in Greville Street, Hatton Garden, will commit a gross error if they attempt to purify the air of their establishment by adopting the measures prescribed in the motion "relating to Mr. TWEEDIE," to be submitted to their notice on Tuesday next. There is another way of doing *that*. Mr. TWEEDIE has in the most straight-forward, prompt, and honourable manner atoned for the exceedingly incautious act which want of experience allowed him,—in common with Mr. J. H GREEN, Mr. BRANSBY COOPER, and some other exceedingly "orthodox" but older and therefore less excusable gentlemen,—to commit, and it would be a most unjust proceeding, after that step, to inflict the supposed punishment on Mr. TWEEDIE. Let the point be judged without prejudice; against the charge let the atonement be weighed; and we are quite sure that the instigations of no uncharitable and mistakenly interested persons will overbalance the just inclinations of the honourable-minded arbitrators. The chief parties in the affair should be advised to hesitate about pressing the matter any farther. The candour of Mr. TWEEDIE, in the admission of his error, and his attempt to redeem it, are, unless we be much deceived, worthy of imitation on the part of the gentlemen who may be his opponents. But *why* his opponents *now?*

The *Birmingham Journal*, of Feb. 13, contains a copy of a memorial presented to the King from the governors, trustees, and medical officers, of the Birmingham School of Medicine, containing a prayer, that "in "order to give additional weight and efficiency to the institution," his Majesty would become a patron of the school, and allow it to be styled the "Royal School of Medicine and Surgery of Birmingham." The King consented, through Earle Howe, and at a subsequent meeting of the memorialists, their "humble and grateful acknowledgments" were unanimously voted to be "laid at his Majesty's feet," for his condescension on this occasion.

From a Correspondent. It is generally rumoured at the London University and Hospital, that it is the intention of the Council of the College of Surgeons to grant to the late pupil of Mr. LISTON, a public examination. Their announcement of the fact is awaited with anxiety.

CORRESPONDENTS.

THE session of Parliament having commenced, it is earnestly requested that all communications to Mr. WAKLEY may be addressed to him at his residence, 35, BEDFORD SQUARE.

Mr. ——. Nothing could have been more spirited or proper than the application to the Board of Guardians, and the refusal &c., under the circumstances stated.

ERRATA.—Page 787, line 15, for *months* read *weeks*.—Line 40, for *liniments* read *lavements*.